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ORIGINAL COMMUNICATIONS.

EXPERIMENTS UPON DIGESTION.

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(Continued from July Number.)

The next question to be decided was, in regard to the influence of the gastric juice upon various alimentary substances, to wit: *albuminous, amylaceous, and oleaginous*.

It is very commonly accepted among physiologists, that the main business of the gastric juice is to act as the solvent of the albuminous, or nitrogenised articles of food, all the other alimentary principles either undergoing no change, or at farthest only a mechanical sub-division. It was reserved for Mialhe to show, that whatever albuminous aliment was presented to the solvent, it not only underwent solution, but was converted into a thoroughly new substance, a low form of albumen, and named by him *albuminose*, or, as Lehmann has proposed to call it, *Peptone*.* The formation of peptone depends solely on the action of the gastric juice, and occurs without the evolution or absorption of any gas, and without the production of any secondary substance.

Whatever article may have been selected for the production of peptone, the following properties may always be recognised. When reduced to the solid form by careful evapora-

* Phys. Chem. vol. ii. p. 50.

tion, peptone is a white, or yellowish-white substance; almost tasteless and inodorous; very soluble in water; but insoluble in alcohol of 83 per cent. Its watery solution reddens litmus, and is precipitated by chlorine, tannic acid and metallic salts, but is unaffected by boiling, by acids, or by alkalies. No precipitation or turbidity is produced by the addition of mineral or organic acids, either in a concentrated or in a very dilute state; even chromic acid fails to produce any appreciable effect.*

The *albuminose*, or *peptone*, thus formed by the action of the gastric juice, is not only soluble independently of the gastric juice, but is also capable of absorption, a circumstance which does not obtain in regard to albuminous substances generally, which penetrate animal membranes with difficulty, their diffusion equivalent, according to Graham, being exceedingly low, sugar being $8\frac{1}{2}$ times and kitchen salt 19 times greater than albumen. On this lies the ground for the observation, that protein compounds, apparently all ready for use as nutriment, require a change by the digestive fluids previous to absorption. Without the gastric and intestinal juices, even soluble albumen and casein are taken up in far too little quantities to suffice for the nourishment of the frame.†

Ex. 10. As the representative of the *albuminous* class, four ounces of rarely done beefsteak were given to St. Martin at 10 A. M., May 5th, after a light breakfast of bread and coffee at 6 A. M., of the same day. No fluid was allowed to be taken in connection with the beef, nor any other article of food.

At 12 M., of same day, St. Martin was subjected to examination. On pushing back the fold of mucous membrane which acts as a valve to the fistulous orifice, a considerable amount of fluid was readily distinguishable in the stomach, mixed with bubbles of air; but no solid matter was visible. About a fluidounce and a half of this fluid was withdrawn from the stomach by a catheter, with the effect of producing nausea which precluded the possibility of obtaining more.

The fluid presented the same reaction as all the others experimented upon; s. g. 1009. Numerous flocculi were visible to the

* Op. cit.

† Lehmann, quoted by Chambers, "Digestion and its Derangement."

naked eye, looking like the debris of food and mucus. It was almost entirely inodorous, viscid, and to the taste decidedly acid.

The microscope, as in the other cases, revealed amorphous granular matter, mucous corpuscles, granular cells, and a few epithelial cells; a few transversely striated muscular fibres were also beautifully displayed, some almost uninjured, some broken down and with the sarcois elements liberated. Numerous oil globules were also distinctly visible, and a few fibres of yellow elastic tissue. The bulk of the material consumed as food had undergone entire solution, and had wholly lost its characteristic appearance.

A portion of the supernatant fluid was boiled actively, without, however, presenting the slightest trace of coagulation. The mineral acids had no effect upon it while cold, but when boiled with strong hydrochloric acid, the purple color of the protein bodies was distinctly manifested. The addition of acetic acid rendered the fluid rather more clear than before. The action of alkalies upon the fluid was not tried; but Trommer's test gave no evidence of the presence of glucose.

A portion of the dissolved material had doubtless been absorbed, but the quantity that still remained, over and above that withdrawn, was not the least remarkable circumstance in connection with the observation. All recent observers agree in stating, that the quantity of gastric fluid poured into the stomach is many times greater than that of the substance to be dissolved; this fluid, however, it must be remembered, is rapidly absorbed again, taking into the blood with it those alimentary matters which it has just dissolved and converted into peptone.

The conclusion from this observation is, that *the gastric juice is a true solvent for animal food.*

The *oleaginous* class of aliments was but imperfectly represented by the fat more or less commingled with the muscular fibres of the food administered as above. The number of oil globules visible under the microscope seems to confirm the observation of Bernard, that fatty matters undergo no change in the stomach beyond that of disaggregation.

Opinions are more divided as regards the influence of the gastric juice upon the *amylaceous*, or starchy articles of food. It

is stated by Mialhe,* that this class of alimentary principles is converted into glucose by the digestive process, and that the saliva is the agent concerned, the conversion being continued even after the starch has descended into the stomach. In this opinion he is sustained by Lehmann, who asserts that grape sugar may be detected in the stomach in fifteen minutes after swallowing balls of starch, or after their introduction through fistulous orifices.† Carpenter also states, that the conversion of starch into sugar may go on in the stomach, but he quotes Frerichs as authority for the belief that it depends on the action of the saliva swallowed, the change not taking place when the œsophagus was tied so as to prevent the deglutition of that secretion, (*Human Physiology*.)

Bernard, on the other hand, denies that any such conversion takes place in the stomach, asserting that the acidity of the gastric juice arrests it, if it have already commenced in the mouth during mastication, or prevents it if it be introduced through a fistulous orifice.‡ Dalton holds the same opinion, which he maintains by experiments with gastric juice obtained from dogs, and by the introduction of starch into the stomachs of these animals through fistulous orifices.§ Both these latter observers, however, state that starch when acted upon by saliva out of the body, is converted into glucose, provided the saliva retain its alkalinity, but that the admixture of gastric juice arrests or prevents it if mixed with it. The following experiments may throw some light upon the subject, the amylaceous materials being represented by wheaten bread.

Ex. 11. On the sixth of May, a portion of wheaten bread was given to St. Martin while fasting, which he masticated deliberately and swallowed. In two hours and a half afterwards a portion of the contents of the stomach was removed for examination. The reaction of this fluid was acid, and the microscopic appearances have been detailed already. (*Ex. 1st.*) Suffice it to say, that in addition to epithelial and mucous cells, starch granules, some whole, some broken, were distinctly recognisable.

* Mémoire sur la Digestion, &c.

† Dalton on Gastric Juice, in *Amer. Journ. Med. Sci.*, Oct., 1854.

‡ MS. Notes of Bernard's Lectures, also Donaldson on Bernard's recent discoveries, *Am. Jour. Med. Sc.*, Oct., 1851.

§ *Am. Jour. Med. Sc.* Oct., 1854.

After allowing the fluid to stand until it had settled, a portion of the supernatant liquid was tested with iodide of potassium and nitric acid, with the effect of manifesting decided evidence of the presence of starch by the production of the characteristic blue color. The same reaction was produced with the tinct. of iodine.

Another portion of the same fluid was subjected to Trommer's test; (solution of sulphate of copper and liquor potassæ;) the result showed the brick-dust red precipitate from the reduction of the oxide of copper, in very considerable quantity.

It may be objected to the above experiments, that dextrine or other organic matters of the fluid were instrumental in producing the reduction of the oxide of copper. ("Cette reaction est une oxydation également commune au sucre, à la dextrine, à la gomme, et à l'alcool. Il est utile d'ajouter encore que l'acide urique, l'urée, et l'albumine peuvent reduire ce reactif.")*

To meet this objection the fluid of digestion of bread was carefully filtered through animal charcoal, by which, as Bernard has shown, all matters, except glucose, are detained. The filtrate when subjected to Trommer's test, again afforded the brick-dust precipitate in large quantities.

Ex. 12. In order to ascertain, if possible, what effect the saliva might have had in producing the glycogenic change in the bread masticated, a portion of bread moistened with water was introduced through the fistulous orifice, and St. Martin was requested to swallow as little saliva as possible, which, as he used tobacco, he had little difficulty in complying with. In an hour and a half afterwards, the contents of the stomach were withdrawn. The same acid reaction was manifest, the same microscopic appearances, and the same solution of the materials were present, although not to the same degree as when the bread was masticated.

The fluid was carried through the same test with a like result, viz: faint evidences of starch and decided evidences of glucose.

Those who have read the admirable paper of Dr. J. C. Dalton, on "Gastric Juice and its Office in Digestion,"† will remember that he states, that "the presence of gastric juice interferes with Trommer's test for grape sugar," and further, that in animals

* Bernard, *Leçons de Physiologie Experimentale Appliquée a la Médecine*, 1855.

† Amer. Jour. Med. Sc, Oct., 1854.

fed with starch, "no sugar is to be detected at any time" in the product of digestion, and still further, that if gastric juice be added to a mixture of saliva and starch, "there is no trace of sugar, even at the end of three hours, and the starch retains its usual properties." Dr. Dalton's experiments were performed upon dogs, in whom he had established fistulous communications with the stomach. It is possible that the greater acidity of their gastric juice may have prevented the metamorphic change, as the writer has witnessed in saliva acidulated with hydrochloric acid; but in the human subject, if we are to accept St. Martin as evidence, his observations have not been verified, indeed they have been disproved.*

The *conclusions* from the foregoing experiments are, that starchy materials are digested in the human stomach; that human gastric juice does not prevent the conversion of starch into grape sugar; and that this conversion may take place in the stomach, independently of the action of saliva, for, as Bernard has shown, any mucous membrane and some alkaline fluids, as the serum of the blood, possess the same power.

In presenting these experiments to those interested in such matters, it is not claimed for them that they are decisive; they are frankly offered as a contribution towards settling a "*quæstio vexata*." Of their value, others must judge. Their prosecution was a subject of deep interest to the writer, whose chiefest regret in relation to them is, that the opportunity was too fleeting to enable him to obtain more decided and extensive results.

Cases of Intermittent Fever. By W. A. PECK, M. D., Berwick, Pennsylvania.

CASE I.† Mary W——, a hale German girl, of 18 years, on the 25th of August, 1855, had an attack of uncomplicated remittent fever, which readily yielded to the usual remedies. This fever was succeeded, after a week's convalescence, by paroxysms of a retarding quotidian intermittent. These she continued to

*It may be mentioned in passing, that cane sugar was also converted into grape sugar in St. Martin's stomach. In the product of digestion of calves' feet jelly, glucose was distinctly recognisable. The gelatine was unchanged.

† Reported in the Boston Med. and Surg. Journal, in Vol. LIII. No. 16.

have, presenting nothing at all remarkable until the fifth paroxysm, when I was called in to witness its singularity. I found her with her right side, arm and leg, shaking most valorously, *while her left side and limbs presented their usual phenomena.* The right side was extremely cold, with *cutis anserina*; small, quick and accelerated pulse, with the other usual characteristics of the ague. The median line very accurately defined the extent of the morbid action. There was, however, a shading off of the coldness of the right, to the natural warmth of the left side.

The skin of the left side presented its usual temperature and moisture; the pulse was much fuller than on the right side. The motor and sensory functions, though perverted on the right, were on the left side perfectly normal. There was slight tenderness in the right hypochondrium, and in the epigastrium, which was the only accompanying symptom discoverable worthy of note. The chill lasted about two hours, when it was regularly succeeded by the hot stage. The right side went through the whole succession of phenomena, as regularly as though both sides were companions in misfortune.

The pulse of the left side partook, of course, of the acceleration of the right, but had not its fulness and tension. The temperature of the skin was slightly elevated, which provoked a moderate perspiration. The fever of the right side went off in the course of four hours, with a profuse sweat. She never has presented any lateral derangement of the cerebro-spinal axis before or since, though she has had the ague repeatedly, and has uniformly brought "all fours" into service.

CASE II. Mrs. O——, a plethoric, active woman, of 30 years, had been subject to intermittent fever, either in its primary form, or, as consecutive to remittent fever, from her childhood. About five years ago, she had a violent attack of remittent fever, which was not arrested until the third paroxysm, which resulted in a *partial anæsthesia of the right side.* I am unable to get a satisfactory history of this paroxysm from her attending physician, further than that the "nervous system seemed to be entirely overwhelmed." Since this time, she has repeatedly suffered from attacks of intermittent and remittent fever, and with an uniformity of results. Her paroxysms now were very like

the one described in case first, *i. e.*, the side normally innervated were not only colder in the cold stage, but hotter in the pyrexial, and perspired more freely in the sweating stage. She has since died, in the third paroxysm of a bilious fever, which, from the beginning, presented no evidence of disease of the right side whatever, save the anæsthesia. The two first paroxysms were similar to the latter, save in the completeness of the paralysis. Aside from the peculiarity already mentioned, there were no symptoms of an anomalous character. It is to be regretted that a *post-mortem* was not permitted.

CASE III. Jas. McN——, an Irish laborer, 45 years of age; suffered repeated attacks of *fever and ague*. In the midst of a succession of these, he was seized with a spinal meningitis of the lumbar region, which afterwards assumed a chronic form, when, by degrees, it resulted in complete paralysis of the inferior extremities.

Before, however, the paralysis became complete, he had an attack of *ague*, which, like case second, affected the parts normally innervated more than the paralytic members. After the complete palsy of the inferior members, neither the cold nor the hot stage had any impression whatever on them. After a tonico-alterative course of treatment, with cold effusions, and a succession of blisters, issues and setons, the man recovered, both from his spinal affection and paralysis; and ever after enjoyed all the benefits to be derived from having the *ague* on “all fours.”

CASE IV. Another case occurs to my mind of precisely similar character. The subject was a feeble girl, of 11 years of age, who came under charge in August, 1855, for treatment of spinal meningitis in the lumbar region, and an approaching paralysis. Like case 3d, she had *ague* before the accession of palsy, and of the usual type; like him, too, had *ague* when the paralysis was incomplete and complete, and finally, after she had recovered from it, and with the same results.

CASE V. This was one of a Swedish collier, aged 40 years; robust and hearty, until by a fall he injured his back, which was subsequently followed by effusion and consequent paralysis of the inferior extremities. The sequel of this case is so exactly similar to the results of the preceding cases, both in the termi-

nation of the case and its behavior in the presence of an intermittent, that it is unnecessary for me to add more.

The foregoing cases, though briefly described, are sufficiently explicit, I trust, to accomplish the object I have in view in their record. These cases have all occurred under my own observation, and I doubt not that many practitioners, who have had much to do with miasmatic fevers, could add many more.

The pathological phenomena presented in the above cases, afford us the most positive evidence that the proximate cause of paroxysmal pyrexia, is the cerebro-spinal axis laboring under the influence of miasma. Hence it is entirely unnecessary for us to formally notice the wild guesses of Willis, Deleboe, Borrelli, and Torti, as to periodical fermentations, acrimonious secretions of the pancreas, &c.; or for a moment entertain the views of Boerhaave and Stoll, or of Selle and Frank as to the unknowable action of the nervous system in the production of agues. It is entirely unnecessary for us to review the characteristics of fever for a satisfactory explanation of the *modus operandi* in producing it. Nor, indeed, need the doctrines of Broussais, Boisseau, Mongellaz, Elliotson, and others, detain us for investigation; for if the vivisection of the *portio dura* demonstrate that nerve to govern the general movements of the face, as the excito-motor acts of a decapitated animal prove the spinal cord to be an independent centre, or the division of that centre shows that its integrity of structure is positively essential to the normal action of the sensory-motor apparatus, then certainly we can as confidently assert, from the testimony of the above cases, that paroxysmal fevers of a malarious origin are primarily affected, modified and controlled by the spinal marrow. For, if an arrest of the normal action of a part of the cord also arrests phenomena which persist in parts normally innervated, certainly the conclusion is unavoidable that the *cord* is the instrument through which malaria acts.

Indeed, an examination of the disposition and physiognomy of this class of fevers is sufficient to convince us that these cases are not anomalous freaks of diseased action, but precisely what we might, *a priori*, expect. The first great character which distinguishes them from the other of the *febres*, *i. e.*, *periodicity*, suggests a community of origin with the same phenomenon

witnessed in *neuralgia*, *epilepsy*, *hysteria*, *insanity* and other affections, of whose *habitat* there is no question, as well as with the divers physiological periodical cycles, which unquestionably acknowledge a neurotic origin. The fact that all cases of periodicity are produced by successional modifications of the nutritive function, affords no valid argument against this conclusion; since these very organic changes are wrought in the organs which manifest these phenomena, and constitute the proximate cause. And it matters not whether these changes were produced by the operation of the "general vital stimuli," or by the agency of *materies morbi*, as long as the instrument continues to be the *cerebro-spinal axis*.

The premonitory symptoms of fever, *i. e.*, pain in the head and back, feelings of weariness, stretching, yawning, &c., very generally point to the spinal axis as the seat of disturbance. In the *rigors* of the "cold stage," we have another example of sensory motor derangement; for the sensation of cold is by no means uniformly associated with actual coldness of the surface, but on the contrary, it is often quite the reverse. In the "pernicious" form of fever, one of the most striking of its phenomena is the doleful lamentation concerning the increased heat, whereas, the surface may be intensely cold. The somewhat characteristic chilly, longitudinal lines, certainly are as capricious as any of the protean *didoes* of hysteria. Besides these, we have the neuralgic pains of the head, back, loins and extremities; occasionally drowsiness, stupor and coma, or the mind morbidly excitable, confused, dejected, or by times wandering. There are other symptoms of the cold stage which depend upon the actually depressed state of the organic functions; but as they stand in no uniform relation to those above mentioned, they must be presumed to be consecutive sequences of the proximate cause.

There are likewise symptoms of the hot and sweating stages, which cannot be attributed to *pyrexia*, as they stand in no uniformly commensurate relation with it, only such as both classes would necessarily bear to the intensity of the cause. Such are pain in the head, back and limbs; convulsions in children; delirium, coma, spasms, &c., &c. The phenomena presented by *pernicious* intermittents also plainly set forth morbid innervation as their proximate cause.

Thus, in uncomplicated paroxysmal fevers, we have sufficient evidence for drawing the conclusion that, at all events, the spinal cord is an instrument through which miasma acts to produce a portion of the symptoms peculiar to the disease. But fortunately we can not only with certainty assert that the spinal cord is affected in these fevers, but that it has a most controlling influence over the nature, severity, and duration of the paroxysm; and not only so, but that its normal integrity of structure and function are positively essential, in order to the production of a complete paroxysm of fever.

The queries very naturally arise, whether the cerebro-spinal nervous system refuses to respond to the reaction of the organic system under the influence of miasm; or, is this system primarily affected, and the action of the sympathetic consecutive; or, are both systems primarily involved in the production of the various phenomena of fevers? That the organic system is concerned in the production of fevers, we must freely allow. That both systems are not primarily affected; must also be sufficiently evident, from the fact, that in paralysis of the sensory-motor apparatus, the parts thus normally innervated present none of the phenomena of fever; whereas, were this the case, it would be difficult to conceive the reason for the absence of derangements in such organs and functions as the sympathetic is known to be able to control. Hence the question resolves itself into this, which system of nerves is dependent for its action in the production of intermitting fevers upon the other for the essential condition of its action?

Now it must be apparent, that were the sympathetic nerves primarily affected, that in case of paralysis of the spinal medulla, there would certainly be an absence of the *perverted sensations* of cold and heat, of neuralgic pains, and of the various other derangements of the sensory-motor function; but there could not, by any known possibility, be an absence of actual coldness in the cold stage, depression of the circulation and general embarrassment of the organic functions; nor of actual heat in the hot stage and fever in its access; nor of sweating in its appropriate stage, since these very phenomena are, to a great extent, under the governance of this system. That such is the case, the evidence already cited abundantly proves; and it further-

more compels us to admit the subordination of the organic to the cerebro-spinal system in the production of miasmatic fevers. Or, that the spinal nerves are primarily affected, and that the ganglionic derangements are consecutive to, and dependent on the reactions of the cerebro-spinal axis.

To this view of the *modus operandi* of *miasma*, it has been objected, that the spinal system of nerves does not supply at all (or but partially) nervous influence to organs especially affected; as the heart, bloodvessels, the secretory viscera, lungs, &c. Granted; but does not the ganglionic system do all this? If so, it is all our theory requires! Again, it is said, that the chief avenues to the system open to the invasion of exciting causes, are the organs of sense and the cutaneous surface; and consequently, if this were the system primarily affected, these causes either could not gain admission, or else the phenomena of intermittents would be referred to these organs! A sufficient reply to this objection is simply the expression of the fact, that the properties of agents capable of exciting the organs of sense, and centrically affecting the spinal cord, are neither identical, nor yet similar. Thus, strychnia undoubtedly expends its energies centrically upon the spinal marrow, which would be a great mistake of the nervous system, if the above doctrine of the necessarily excentric action were true.

Neither is this theory materially affected by the assertion that the total annihilation of the cerebral functions would not produce death as soon as miasmatic action is known to do in some cases. Now it is worth while to recollect, not only that we do not claim paralysis to be the proximate cause of fever, but the fact, that to our senses, the impression produced upon one system of nerves is instantaneous with its repetition on the other. Again, it is claimed, that the cerebro-spinal nerves can exert no influence over the circulatory system; the production of animal heat, the secernent functions; nor yet over the processes of assimilation and nutrition; *ergo* this system of nerves cannot be the only system primarily affected. But again, I must insist upon the consecutive action of the sympathetic nerves as an explanation for the phenomena peculiar to these functions in fever.

But the most potent objection that has been urged against this theory is, that the normal influence of the spinal cord over

the organic nerves is insufficient to warrant the conclusion that when under the influence of malaria, it can so far control the last named system as to produce the symptoms attributed to it. This objection, if founded in fact, would successfully invalidate our theory; but the evidence now adduced, shows the inference to be unfounded, and hence speaks in more positive and authoritative terms than any man's *opinion*, from the present state of our knowledge, has a right to assume.

Nor is intermitting fever an isolated example of this influence over the sympathetic system. An error of diet, teething, worms, passage of a catheter, and many other irritating agencies, have all and each raised a very noticeable commotion throughout the economy.

Chorea, its Nature and Treatment. By O. C. GIBBS, M. D.,
of Frewsburg, Chataque County, New York.

The involuntary jerking movements and imperfect subjugation of the muscular powers to the control of the will, which characterize chorea, entitle it to a place among convulsive diseases of the nervous system. It is true, it is less serious in its import than tetanus, or epilepsy, yet it is of sufficient interest to merit great attention, and has with those diseases many symptoms in common. In epilepsy there is loss of consciousness, and consequently of volition, with convulsive spasms. In tetanus there is rigid spasm, with no defect of volition or suspension of consciousness. In chorea, as in tetanus, consciousness and volition are intact; there is no deficiency of muscular energy, but that energy is exercised by a power independent of the will, and but imperfectly subject to it. In epilepsy the convulsions are paroxysmal, or intermittent; in chorea the jactitations are persistent, excepting, perhaps, during sleep, it may be for weeks or months, or, perhaps, for a lifetime. In tetanus and epilepsy the convulsive tendency is towards aggravation; in chorea, fortunately, it is generally towards amelioration and recovery.

Chorea is essentially a disease of early life, occurring mostly between the ages of five and fifteen. It is not, however, exclusively a disease of youth, as, perhaps, no period of life is exempt from its attacks. It occasionally comes on for the first

time during pregnancy, in which event it usually persists until after delivery. (*See Guy's Hospital Reports*, vol. vi. Part 2. Cases 26 and 27.)

According to the statistical observations of Ruz and others, the disease is more common in females than in males, in the proportion of about three to one. This may be accounted for on the ground of their known increased nervous susceptibility.

As we do not propose, in this brief paper, to enumerate the symptoms, diversities, and causes of chorea, we pass directly to a few observations upon its nature.

As death seldom takes place from the uncomplicated disease, the researches of the pathological anatomists have been made under unfavorable circumstances. The cerebrum, cerebellum, and spinal cord, have been repeatedly and thoroughly searched for the causative lesions upon which chorea depends. Various lesions have been observed; but, so diversified in character and so manifestly connected with complications, as to justify the conclusion that they were unconnected with the choreic symptoms. Hence, the researches of the pathological anatomist have revealed no positive light, in reference to the nature of the disease under consideration. Physiologists have attempted to locate a lesion, which, from its nature, must, perhaps, forever escape the vision of the anatomist, but so far, we believe, without uniformity of result.

The pathology of chorea, has been the subject of much controversy; yet, it is admitted by all, that the primary irritation is in the nervous system, but whether in the brain or medulla spinalis, is a question upon which the controversy hinges. Marshall Hall believes that the primary irritation is in the spinal marrow; Todd and others refer it to the brain itself. In the absence of the post mortem evidence, an opinion, in reference to the seat of the primary irritation, must be based upon the physiology of the nervous system, as productive of muscular motion, whether connected with or independent of volition. All *voluntary* movements, all acts of volition and sensation, receive their stimulus from the brain, and cannot take place without that organ. The *involuntary* movements do not of necessity involve sensation, and are performed without any participation of the will, it may be in opposition to it; they are called forth by

stimuli applied to the peripheral extremities of the afferent nerves and reflected through the spinal cord. The physiology of the nervous system, as connected with volitional and automatic movements, is differently explained by different physiologists. Dr. Todd, differing somewhat from Dr. Hall and also from Dr. Muller, gives expression to his hypothesis in the following language, which we cannot do better than to quote. He says, "All the spinal and encephalic nerves, of whatever function, are implanted in the grey matter of the segments of the cerebro-spinal centre with which they are severally connected, and do not pass beyond them. The several segments of the cerebro-spinal axis are connected with each other through the continuity of the grey matter from one to another, and through the medium of commissural fibres which pass between them. Through these means, motor or sensitive impulses may be propagated from segment to segment; and stimulus conveyed to any segment from the periphery may either simultaneously affect the brain or cause a sensation, or it may be reflected upon the motor nerves of that segment, and stimulate their muscles to contract. Or both these effects may take place at the same moment, as a result of one and the same stimulus. According to this hypothesis, each segment of the cord, so long as it retains its proper commissural connexion with the brain (by commissural fibres and continuous grey matter), is part and parcel of the centre of volition as well as that of sensation, and the mind is as directly associated with each segment of the cord as it is with any portion of the encephalon. Let that commissural connexion be dissolved, and the mind will immediately lose its hold upon the cord; but the various segments of that organ may nevertheless still be acted upon by physical impulses, and may still continue to evolve the nervous force in connexion with the natural changes which may take place within." (*Physiology of the Nervous System*, p. 38.) The mechanism of *voluntary* motions, in parts supplied by spinal nerves, he thus explains:—"The impulse of volition, excited primarily in the brain, acts at the same time upon the grey matter of the cord (its anterior horn), and through it upon the anterior roots of the nerves implanted in it. This grey matter, in virtue of its association with the brain by means of the anterior pyramids, becomes a part and parcel of the organ of the will,

and therefore as distinctly amenable to acts of the mind as that portion which is contained within the cranium. If we destroy the commissural connexion with the brain through the pyramidal fibres, the spinal cord ceases to take part in mental nervous actions; or, if that connexion be only partially destroyed, that portion of the cord which the injured fibres had associated with the brain, is no longer influenced by the mind. Again, if the seat of volition in the brain be diseased, the cord, or part of it, participates in the effects of the disease as far as regards voluntary actions." (P. 47.)

Dr. Todd, in his "Lumleian Lectures" for 1849, on the pathology and treatment of convulsive diseases, published in the "Medical Gazette," says, "the brain and spinal cord as the great centres of the nervous system—the great nervous battery of the body—show distinctly a division into two portions: one in which nerves are implanted; the other which has no immediate connexion with nerves, and communicates with them only through the medium of the former part." (*Lecture 2*, p. 726.) Now, if we rightly understand Dr. Todd, in the extracts given above, and we restrict the brain proper to that portion of the intracranial mass which has no connection with nerves, we can hardly see how the primary irritation in chorea can be referred to the brain itself. With Dr. Todd's view of the physiology of the nervous system and of muscular motion, the arguments of Dr. Carpenter and others, in proof of the purely cerebral origin of chorea—namely, the peculiar connection of the disordered movements with emotional excitement, and their cessation during sleep—seem to us devoid of force. The jactitating and involuntary character of the convulsive movements, performed in opposition to the will and but limitedly under its control, points to some portion of the automatic apparatus as the seat of the primary lesions in choreic convulsions. An able writer thus contradistinguishes the automatic and cerebral actions: "The sensorial ganglia, medulla oblongata, and spinal cord, are the immediate instruments of all sensorial and motor changes: by their sole and independent action are produced all those movements, which, being unprompted by the will, or even taking place in opposition to it, are said to be automatic. These automatic movements may

be excited either by sensation, or by impressions which do not necessarily affect the consciousness."

"On the other hand, the cerebrum is the instrument of purely mental operations, and has no direct connexion with the external world. It receives all its stimuli to action through the sensorial ganglia; and it exerts all its influence upon the muscular system through the centres of automatic movement. Hence it may be said to be played upon by the sensorial portion of the automatic apparatus, and to play upon its motor portion."

"The will may be exerted in antagonizing, strengthening, guiding, or controlling the automatic actions; but in every case, the muscle directly receives its power from the automatic centre; and the very same movement may be automatic, emotional, or voluntary, according as the motor influence has been excited in the centre from which the nerve proceeded, by an external stimulus acting through an afferent nerve, or by an emotional impulse transmitted downwards from the sensorial centres, or by a volition originating in the cerebrum." (*Medico-Chirurgical Review*, Vol. 5, pp. 17 and 18.) Admitting this view of the cerebral and automatic actions, and drawing the line, as above, between the brain proper and the automatic apparatus, the chain of sensory ganglia, at the base of the brain, can only be regarded as the compliment of the spinal cord, in the formation of the automatic centre on which the cerebrum is superpoised. Having then established the fact that choreic convulsions are excited by irritation, primarily within the automatic centre or reflected to it from the peripheral extremities of afferent nerves, the next inquiry is, whether that irritation is within the spinal cord, medulla oblongata, or sensorial ganglia. Choreic convulsions are occasionally succeeded by partial paralysis and hemiplegia; from this fact, Dr. Todd argues that the seat of the irritation is intra-cranial and above the decussation of the anterior pyramids. He locates the primary irritation in that portion of the sensorial ganglia which is most nearly connected with the points of implantation of the auditory and optic nerves. This opinion very nearly harmonizes with that expressed by Dr. Carpenter; and, though located within this complimentary portion of the spinal cord,—the sensorial ganglia,—the topmost portion of the automatic centre, it is not within the spinal cord itself, as supposed

by Dr. Hall. If additional arguments are wanted, aside from the one given, namely, the frequency of choreic hemiplegia, to prove that the primary irritation is not in the spinal cord, we may mention the frequent origination of chorea from mental causes, and also the cessation of the characteristic jactitatory movements under the influence of sleep. That the seat of irritation is not in the brain proper, derives additional confirmation from the fact that the consciousness is not affected even in the severest forms of the complaint.

Though physiologists have determined the seat of the primary irritation with a good degree of probability, yet we have no reason to expect that the pathological anatomist will ever find there, as a choreic cause, any serious organic lesion. In fact, it is next to certain, that no such lesions exist. In tetanus, whether resulting from its own natural poison or artificially induced by strychnine, there is a greater disturbance of the automatic centres than in chorea. In tetanus, unlike chorea, deaths frequently occur uncomplicated by other diseases. Yet in death resulting from tetanus, pathological anatomists have so far failed to detect any evidences of organic lesion in the cerebro-spinal centres, and we have reasons for concluding that, if the severe tonic spasms of tetanus are independent of organic lesion, the milder jactitations of chorea are no less so.

What is the nature of this irritation, seated probably in some portion of the sensorial ganglia and causative of chorea? It is said that the affection may be produced by intestinal irritation, such as worms, constipation, vitiated secretions, &c.; by fright, cold, &c.; but, is it not probable that a choreic idiosyncrasy or diathesis must previously exist? Cold and catarrh are numbered among the causes of phthisis, but a tubercular diathesis must previously exist; the local disease is the result of a constitutional cause, involving the nutritive function and stamping that function with its peculiar idiosyncrasy. Gout may succeed a debauch, but without the pre-existence of a gouty diathesis, tainting the circulating system with its poisonous products, the debauch would prove inert, so far as the development of the painful local symptoms is concerned. Though choreic convulsions result from an irritation in the sensorial portion of the automatic apparatus, yet it is highly probable that this irritation

is preceded by a constitutional affection—a choreic diathesis, in some way involving the blood, resulting in depraved nutrition, and a weakened and irritable condition of the automatic centres. Dr. Todd supports this view of the nature of chorea:—upon this point, he says, “the disease is essentially one of depraved general nutrition.” Dr. Simon says, “chorea confines its attacks to individuals in whom the blood development is obviously defective.” (*Simon's Pathology*, p. 154.) Not only in chorea, but in all diseases where exaltation of function is evinced by the cerebro-spinal centres, as a primary nervous phenomenon, it is highly probable that the irritation in the automatic apparatus, is caused by some influence of which the blood is the vehicle. It has been thought by some, that this choreic idiosyncrasy is identical with, or similar to the rheumatic diathesis. The idea, advocated by Drs. Addison and Todd, is probably predicated upon the frequent accompaniment, in chorea, of a peculiar morbid affection of the heart, manifested by a bellows-sound, commonly mitral, and indistinguishable from that which, in rheumatism, is associated with organic lesion of the mitral valves. This idea of identity, gains some support from the fact that choreic patients are frequently the offspring of rheumatic patients, and their urine is frequently loaded with lithates, as is the case with rheumatic patients. Yet the idea of identity is not established, and may well be questioned. Perhaps we cannot better conclude our remarks upon this point than to quote from Dr. Simon. He says, “as regards the affinity between chorea and rheumatic fever, it does not, by any means, appear to me that the humoral disorder is identical for the two diseases, since they are never coincident in their occurrence; but it seems, rather, that the material which collects in the blood prior to the attack of rheumatic fever, and which, by its explosive decomposition, subsequently evolves the immense evacuations of this disease, may, while accumulating within the circulation in its original form, become capable of producing that irritation of the nervous centres which is characteristic of chorea. (*Simon's Pathology*, p. 155.)

In the *treatment* of chorea, there is probably no remedy or set of remedies adapted to all cases; but the peculiarities of each individual case should be attentively studied, and remedial

administration be made accordingly. The causes, whether predisposing or exciting, that may have given development to the disease, should be attentively sought for and removed when practicable. If a disordered condition of digestive organs, vitiated secretions, irritation of the stomach or bowels, constipation, intestinal worms, difficult dentition, &c., &c., be found connected with the disease, as a probable exciting cause, remedies should be judiciously selected for the removal of them. It is, probably, in consequence of the great variety of unhealthy conditions, that predispose to or immediately excite choreic convulsions, that so great a variety of remedies have found enthusiastic advocates with the profession in the disease under consideration. When constipation is a symptom, the bowels should be thoroughly evacuated at once, by suitable purgative medicines, and their regularity of action be secured throughout the period of subsequent treatment, by the daily administration of laxative medicines, combined with such other medicines as the peculiarities of the case may require. To allay irritation in the sensorial ganglia, counter-irritation to the back of the neck, by repeated blisters or the tartar emetic ointment, is a valuable auxiliary to the other remedies employed.

In addition to the instrumentalities mentioned, defective nutrition calls for invigorating remedies; enfeebled nervous actions, responsive to the will, require nervous stimulants and tonics; automatic excitation and jactitatory movements seem to demand nervous sedatives and antispasmodics. To give to all these indications their due significance, without undue medication, requires no little judgment and therapeutic skill. Of the invigorating remedies, iron and zinc are the most deserving of trial. Every one knows the effects of chalybeates in invigorating the nutritive functions and enriching the blood, in chlorotic conditions of the system; and it has been thought, by Dr. Golding Bird and others, that zinc exercised a similar influence on the nervous matter that iron does upon the blood. Be this as it may, the efficacy of zinc in chorea has long been established. Of the remedies directed more particularly to the nervous system, appropriate in the disease, may be mentioned musk, camphor, valerian, assafetida, cimicifuga, &c.; of these, probably, the cimicifuga is of the first importance. Its efficacy was first made

known by Dr. Young, and it has since been recommended by several other eminent physicians of this country. Its *modus operandi* is not understood, but the utility of its administration, when in appropriate doses, is beyond a question. The similarity of some of the symptoms in chorea and rheumatism, finds here a counterpart in the similarity of appropriate treatment, for in chronic rheumatic pains, there is probably no remedy superior to the *cimicifuga*, when perseveringly given in doses sufficient to affect the nervous system, which is evidenced by vertigo, disordered vision, headache and reduction of the circulation.

Of the preparations of iron, probably the ammonio-tartrate, or the ferrocyanuret are equal to any of the others. The ammonio-tartrate possesses the advantage of perfect solubility with an agreeable taste.

The following formulæ are either of them good ones :

R. Ferri ammonio-tart,	-	-	ʒij.
Aquæ cinnamon,	-	-	f. ʒiiij. ℥.

S. A teaspoonful three or four times a day. Or,

R. Ferri ferrocyanuret,	-	-	gr. xv.
Extract valerianæ,	-	-	ʒij ss.

Make into 24 pills; S. one to be taken 3 times a day.

Of the preparations of zinc, the sulphate is the one usually employed. The following is a good formula.

R. Zinci Sulphat.	-	-	ʒi.
Extract gentian.	-	-	ʒij.

Make into 20 pills. One to be taken 3 times a day and gradually increased to six pills a day.

The *cimicifuga* may be given simultaneously with either of the formulæ above. Of the saturated tincture, two fluidrachms may be given three times a day, and it should be gradually increased until it produces sensible effects upon the nervous system, as mentioned above. These doses are appropriate for patients at or beyond the age of puberty, and should be diminished to correspond with ages below that.

The therapeutic bibliography of chorea is rich in variety; we give below a few of the more prominent instrumentalities of cure, that at different times have had enthusiastic advocates.

Blood-letting was recommended by Sydenham and Cullen, as well as by equally authoritative names in later times. But

since it has been found that chorea is an affection connected more or less with diseased or defective nutrition, venesection has gradually, and with good reason, gone out of favor. It is now practised by but few, and then only in the acute form of the disease. The violence of the symptoms in general chorea has tempted many to use the lancet, but it has been justly observed by Dr. Todd, that the more you weaken the patient under these circumstances, the more incessant and urgent will the movements become.

Cathartics have been advised and strongly insisted upon in this disease by almost every writer, from the days of Sydenham to the present time. Connected as the disease nearly always is with constipation and deranged secretions, purgatives have been looked upon as an indispensable remedy, and some have considered them the only ones required. We apprehend constipation to be not so often the exciting cause of choreic convulsions as many suppose, but rather a legitimate sequence of a pre-existing choreic condition. In all disease of the brain or spinal marrow, in apoplexy and epilepsy, in coma and convulsions, constipation is usually a symptom, and we see no good reason why in chorea it should be anything else than a symptom. As such it certainly deserves attention; a cathartic should be given at first, and the bowels kept regular afterwards by suitable aperients, but purgatives should not be wholly or principally depended upon as curative agents.

Opium was employed by Sydenham and strongly advised by Cullen, and has since been used as a nervous sedative by many physicians, but with no very satisfactory results. The cold bath is a better sedative, and possesses the advantage of invigorating the system when judiciously and perseveringly used. The testimony of Dr. Todd bears directly upon this point. He says, "from what I have myself seen, I would say, that the greatest immediate benefit arising from the use of the cold bath is from its exciting a disposition to sleep much more decidedly than opiates, which generally do not act favorably in any of the forms of chorea." There are kinds of wakefulness, as well as of jactatory movements, over which opium seems to have but little control, and deaths, doubtless, have occasionally occurred in the laudable attempt to produce, with this agent, results

which it had not the power to accomplish. In the second volume of the American Medical Monthly, Dr. Hibben, of Brooklyn, communicates a case of death in a choreic patient, which death was to him as unexpected as it was unaccountable. When we reflect that this patient took "grain doses of sulphate of morphia, every two hours for days together; also a suppository R. morph. sulph. gr. iij., butter of cocoa, q. s." while the bowels were for six days confined, we can scarcely resist the conviction, that if death did not immediately result from medication, there was, at least, a better way to have met the indications.

Chloroform has been recommended as a sedative in very bad cases, and doubtless its administration is frequently of service. Dr. Barclay, in the Medical Times and Gazette, for 1853, reports a case in which arsenic, iron, cathartics, opium, &c., had failed to produce relief, but which, when the patient was much exhausted by the constant jactitations and want of sleep, was immediately relieved by the inhalation of chloroform, and ultimately cured by its occasional repetition, in connection with the regular administration of eight grain doses of quinine in suppository.

Antimony was advised by Breschet in as large doses as the stomach would bear without vomiting. It has been occasionally employed ever since; but seeing no indications that it is calculated to fulfil, we think it may well be dispensed with.

Strychnia was employed by Trousseau, and in the majority of cases with complete success. That benefit may result from the administration of this remedy we have not a doubt, but we should expect more decided results from it in chronic and hemiplegic cases than in any other.

Electricity has been highly commended by Drs. Bird, Addison, and others, and success claimed for it scarcely second to that of any other agent. Cases adapted to its use are probably similar to those in which strychnia is of service. This opinion is advanced with some degree of hesitancy, for it is only by more extended and scrutinizing trials that cases appropriate for either of the last two mentioned agents can be determined.

Arsenic has had many advocates. As a mineral tonic in chorea it is second only to iron and zinc, and in certain cachectic conditions of the system, or in patients whose healths have

severely suffered from miasmatic influences, it is probably even superior to these. We presume that in conditions like the last supposed, that quinine has in some instances proved of such marked advantage.

Many other remedies have been proposed, and some of them recommended by high authority, among which may be mentioned assafoetida, camphor, musk, oil of turpentine, nitrate of silver, iodine, hyoscyamus, hydrocyanic acid, stramonium and belladonna.

BIBLIOGRAPHICAL NOTICES.

A Practical Treatise on the Diseases of the Testis and of the Spermatic Cord and Scrotum. With numerous wood engravings. By T. B. CURLING, F. R. S., Surgeon to the London Hospital, &c. *Second American from the second revised and enlarged English edition.* Philadelphia: Blanchard & Lea. 1856.

We are pleased to see another edition of the above excellent treatise. The author introduces it to the notice of the profession in the following terms: "More than twelve years," he says, "have elapsed since the publication of the first edition of this work. During that period, I have continued my enquiries into the morbid changes, which occur in the testicle, and have availed myself of increased opportunities of studying its diseases. In this edition some new chapters have been added; many have been re-written or altered; and, it is hoped, that nearly all of them contain additional facts of practical interest and importance."

We have taken considerable pains to compare the present with the previous edition, and take pleasure in stating, as the result of our investigation, that it is in every way superior to it. In style and precision of language there is a marked improvement, a large amount of new and important matter has been introduced by the author, while his increased experience has enabled him to speak positively on many questions on which his opinions were indecisively or differently given in the first edition.

As it now stands, we can unhesitatingly speak of it as the first work upon the subjects it treats of in our language, one worthy in all respects of the confidence and approbation of the profession.

The first part of the American edition is taken up with the anatomical description of the scrotum and testis, to which is added a short account of the spermatic fluid. In the English edition, this part has been omitted for reasons stated by the author in his preface. "By a different typographical arrangement of the American edition," says the Publisher's advertisement, "space has been found for this valuable section without enlarging unduly the size of the work, and accordingly such portions of it have been retained, as had not been introduced by the author in various chapters throughout the volume."

Under the head of Diseases of the Testis, Chapter I. describes its various congenital imperfections and malformations under the sections of numerical excesses and defects, deficiencies and imperfection of the *vas deferens*, imperfect transition of the testicle and inversion of the testicle. In the section upon the imperfect transition of the testicle, will be found, a very clear and satisfactory description of the mode and agency by which the passage of the testicle into the scrotum is effected. "In some instances, the passage, though delayed, is completed at some period previous to puberty, and often a few weeks after birth." "My own observations," says Mr. Curling, "lead me to believe, that if the passage does not take place within a twelvemonth after birth, it is rarely fully and perfectly completed afterwards, without being accompanied with rupture. For the causes which operate at this late period, tend as much to promote the formation of hernia as the transition of the testicle. In cases where the testicle makes no appearance before puberty, uneasiness is often experienced at that period, owing to the enlargement of the gland being restrained by the rings and parts composing the inguinal canal. At the same time, also, it is often protruded outside the external ring by the movements of the abdomen in respiration." The causes of this failure of the testicle to pass into the scrotum, Mr. Curling attributes to deficient power in the *musculus testis*, to adhesions between the abdominal viscera, the result of peritonitis in the foetus in utero, and to the smallness of the opening

in the external abdominal ring, especially where the testicle is detained in the inguinal canal, in which situation, we are informed, it is more frequently found than in the cavity of the abdomen.

As practitioners are frequently liable to meet with these embarrassing cases, it becomes their duty to be acquainted with the proper methods of treating them. We feel, therefore, we cannot employ our space to better advantage than in copying Mr. Curling's excellent advice, the result of a large experience in such cases:—

“When a testicle is retained in the groin, there are various circumstances which tend to interfere with its evolution at puberty, to impede its nutrition and to excite inflammation and disease in it, and I have shown from dissections that such results are not unfrequent. A testicle, therefore, situated in the abdomen is in a more satisfactory position, and is much less exposed to injury and disease, than one which has been arrested in the groin. On this account, and as the passage is seldom perfectly accomplished when delayed beyond the age of one year, if the gland has not made its appearance at this period, the well being of the patient will be best consulted by the employment of some mechanical means to prevent the escape of the organ from the abdomen. A strong reason for adopting this practice is afforded by the great liability to rupture which exists in all cases of the tardy transition of the organ, owing to the persistence of a sac ready prepared for the reception of a protrusion, and in many instances to adhesions between the testicle and intestine or omentum. A hernia may occur whilst the testicle is still in the abdomen, or after it has passed the ring, and the viscera may descend into the scrotum, the gland being detained in the groin. Cases of this kind are embarrassing, as it is impossible to fulfil the two opposite indications of preventing the protrusion of the viscera, and encouraging the descent of the testicle. Many years ago I had under my care a fine child, neither of whose testicles had made their appearance out of the abdomen. When I first saw him, he was about a year old, and had an inguinal rupture on both sides, which descended whenever he cried or struggled. In accordance with the usual practice, I objected to the application of any truss. The parents became anxious and impatient at the annoyance arising from the hernia, and consulted a high authority, who gave similar advice to that received from me. The rupture was consequently left to itself, and the boy restrained from exercise. He was petted, became fretful, and proved a constant cause of uneasiness to the parents. When I last examined him he was eight years of age, and fortunately the rupture on the right side had disappeared spontaneously, and the one on the left protruded very slightly, but there was no appearance of the testicles. Now, if it be granted that a testicle situated in the abdomen is in a better position than one placed in the groin; that it is productive of less inconvenience, and

exposed to fewer causes tending to impair its structure; that its subsequent passage, if it ever takes place, is frequently, if not commonly, attended with rupture, it must, I imagine, likewise be admitted, that the advice often given in these cases is unsound and injudicious. In recent years I have invariably advised the application of a truss so as to prevent the descent of the testicle as well as the escape of intestine, which I am sure has contributed much more to the health and comfort of the patient, than leaving him exposed to the inconveniences and dangers of an unrestrained rupture.

"In certain cases where the testicle has passed out of the external ring, but without descending fully into the scrotum, complicated with hernia, a truss with a small pad carefully applied may serve to keep up the rupture, and at the same time prevent the testicle from slipping back into the inguinal canal. When this can be done effectually without risk of the pad pressing on the testicle, it is the practice which should be adopted. But if the testicle is constantly gliding in the way of the pad so as to be exposed to pressure, or if adhesion exists between a portion of intestine and the gland, this treatment is inapplicable, and a truss should be applied to keep the parts if possible within the abdominal cavity.—A middle-aged gentleman consulted me on account of a large scrotal rupture on the right side. A great part, which consisted of bowel, could be returned without difficulty, but a mass remained irreducible unless in company with the testicle, and this was clearly made out to be a large portion of omentum adherent to the gland. On forcing up all the parts, I found it impossible to apply a truss without making pressure on the testicle, and more than ordinary pressure was needed to prevent the protrusion of so great a mass. So much inconvenience and risk attended leaving the rupture unrestrained, that I was compelled to apply a truss without returning the omentum, which was necessarily exposed to pretty strong compression from the truss-pad. The pressure led to his suffering occasionally from a dragging pain referred chiefly to the left side, particularly when he was affected with flatulency or distended bowels. The pain was relieved by easing the truss and rest in the recumbent posture. This gentleman had a varicocele on the left side, and wore a double moc-main lever truss, by which he was enabled readily to moderate the pressure."

Under the head of "Diagnosis in Cases of Imperfect Transition," besides mentioning the liability of the testis, when retained in the groin to be mistaken for a bubonocoele, Mr. Curling adds, in the present edition, the following caution:—

"A testicle retained in the groin when inflamed, is liable to be mistaken for a bubo, the prominent oval swelling communicating a deceptive feeling of fluctuation, and being attended with pain; the skin over it occasionally exhibiting even a slight red blush, and the tumor being seated in a region where bubo constantly occurs and suppurates. It is related that Ricord, of Paris, was once very nearly deceived by a case of the kind, and even called for a knife to open the supposed abscess, but

a re-examination of the tumor having led to the discovery of the absence of the testicle on that side of the scrotum, he made further investigation, and detected the true nature of the case."

The next two chapters treat of Atrophy of the Testicle and Injuries of the Testicle. Under the latter head, the author describes several curious cases of self-castration. These cases, it is stated, usually do well, the state of mind under which they were performed not generally operating prejudicially to the patient's recovery.

Hydrocele and its Palliative and Radical Cure, are fully discussed in Chapter IV. In Simple Vaginal Hydrocele, the superiority of injections over all other modes of treatment is strongly endorsed. Of the iodine injection the author speaks most favorably, and rarely resorts to any other:—

"The only apparatus required, in addition to a medium-sized trocar, is a half-ounce glass syringe with a metallic nozzle, which fits into a small stop-cock adapted to the canula. The metallic parts should be made of palladium, which is not acted on by iodine. I employed at first injections of the strength recommended by Mr. Martin (one drachm of the simple tincture of iodine to three of water), but I found this too weak, and I have used latterly a compound tincture of the following strength undiluted,—iodine \mathfrak{Dij} , iodide of potassium $\mathfrak{3ss}$, spirits of wine $\mathfrak{3j}$,—injecting from two to three drachms, and allowing this to remain in the sac for five minutes. The greater part of the fluid is then withdrawn, about half a drachm only being left behind in the sac. Some surgeons are content to inject a drachm of the tincture, and to leave it in the sac, which answers quite as well. I have not found the tincture employed in this way in adults at all too stimulating. In operating, however, on persons under puberty, I dilute it one-half.

"I generally perform the operation on the patient standing, but it may be done equally well in the recumbent position. Directly the stimulating fluid becomes lodged in the vaginal sac, the patient generally feels sick and faint, and experiences pain in the part, and in the cord, with uneasiness in the loins. The pain is sometimes so severe that the removal of the injection becomes necessary before the expiration of the usual period. The amount of inflammation excited by the operation cannot, however, be estimated by the degree of pain suffered at the time. There is great difference in persons in their tolerance of stimuli, inflammation being more readily excited in some than in others, but its amount and intensity by no means depend on the susceptibility of individuals to pain."

It will be remembered that the previous edition advocated the use of lime water as the best fluid for injection.

Of the treatment by the seton, the author appears to entertain

a more favorable opinion than he did at the date of the previous edition. In certain cases he even prefers it to the treatment by injection.

“In cases of simple hydrocele, after the failure of injections by others, I have also used the seton with success, and I have tried it, too, in cases where no other treatment has been adopted. The great objection to its use in simple hydrocele is the uncertainty of its operation. I have generally found it both a sure and gentle remedy, though occasionally I have been disappointed by its producing high inflammation, which it was impossible to control, and which speedily ran on to suppuration.”

Hæmatocele, Orchitis, Tubercular disease, Carcenoma, Cystic-diseases, Fibrous and Cartilaginous Tumours of the Testis, Calcareous and other deposits, Entozoa in the Testicle, Sympathetic and Functional Disorder of the Testicle and Castration, are fully described in the succeeding chapters.

The second part of the work, comprising three chapters, is devoted to Diseases of the Scrotum. The various methods of treating varicocele, the most important of these affections, are fully described by Mr. Curling under the heads of palliative and radical treatment. Under the latter head, division of the spermatic vessels, the ligature, compression and excision, are severally discussed and generally disapproved of, as either being attended with risk of hæmorrhage or phlebitis, or liable to be followed by wasting of the testicle. The principal indication for the perfect cure of varicocele is to relieve the dilated and weakened veins of the weight of the blood lying in and above them, and “so enable them to return to their natural dimensions, and recover their tone so as duly to carry on the circulation.” “The only plan,” says Mr. Curling, in his first edition, “which appears to be fully adapted to effect this object, is firm, steady and continued pressure on the spermatic veins at the ring, by means of a well adjusted truss. At present, our experience of this mode of treatment is too limited to admit of any opinion of its efficacy being confidently expressed; but I look with no slight interest to the result of further trials of a remedy which seems to me to be based on sound views of the pathology of the disease.”

The present work contains the author's further experience of the method, which, so far as we are informed, has proved in every instance in which it has been tried, remarkably successful, several

cases being detailed by him in which the affection was entirely cured by pressure (by means of a moc-main lever truss), in from seven to nineteen months. "From these observations," Mr. Curling concludes, "it will appear, that I consider the treatment by pressure to be applicable either for the cure or relief of the majority of cases of varicocele occurring in practice. Certainly, in all those cases in which tolerably firm pressure with the fingers, at the abdominal ring, removes the sense of weight and uneasiness along the cord, this plan may be resorted to with every prospect of a beneficial result; and its simplicity, freedom from all risk, and efficiency, render it preferable to all operative modes of treatment."

The remaining parts of the work are devoted to diseases of the Scrotum. We had marked several pages for extraction and comment in this portion of the work, but the space over which our article has already extended, obliges us to omit them.

The Publishers' advertisement states that the work has been passed through the press under the superintendence of Dr. Gobrecht, to whom we are also indebted for several additional engravings, and a case of interest. We are happy to say, that in reading the work pretty carefully, we have not detected a single typographical or other error in it. We observe, however, that the few notes introduced into the first edition by its American editor, although no notice whatever has been taken of them by the author in his second edition, are all of them again inserted in the present re-print. We mention the fact, not to disparage their value, but merely to state our opinion that it would have been better, under such circumstances, to have omitted them. The work, we should add, is handsomely printed and illustrated.

Medical Jurisprudence. By ALFRED S. TAYLOR, M. D., F. R. S., &c., &c. *Fourth American from the fifth and improved London edition. Edited with additions.* By EDWARD HARTSHORNE, M. D., &c. Philadelphia: Blanchard & Lea. 1856. 8vo. pp. 697.

We hazard little in affirming our belief that Taylor's *Medical Jurisprudence* is the best *manual* on its subject in any language. It has so long occupied the first rank among our most popular

text books, and is so favorably known to readers of every kind, legal, medical and general, that the mere announcement of a new edition is an all-sufficient recommendation.

The previous efforts of its author and editor afford ample guaranty of continued improvements in each new issue of their work; and we need only to make a very cursory examination of the volume before us to be satisfied that the additions and alterations are both numerous and important, and such as fully to sustain the previous reputation of the manual, and that of its indefatigable author.

"Including the present edition," says Dr. Taylor in his preface to the fifth London edition, now some two years ago, "there have issued from the press, since the first publication of this work in November, 1843, ten thousand seven hundred and fifty copies. The encouragement that has thus been bestowed upon the author for his labors in a difficult branch of medical science, has induced him to revise with care the fifth edition now laid before the profession. The reader will find, on comparison, that considerable additions have been made, amounting in the whole to upwards of one hundred pages." It might be said, with equal truth, that similar encouragement in this country has stimulated editor and publishers to analogous efforts here, and the result is certainly a production which neither party need fear to compare with its predecessors from the same source.

The short interval which has elapsed since the publication of the English edition, and the evident desire of the American editor to confine his annotations altogether to that interval, have left the latter a very limited field of operation in the fulfilment of his duties. These accordingly have been, to use his own language, "principally confined to a careful revision of the text, the incorporation of the addenda, and the introduction of occasional brief notes of recent cases and decisions, and references to others as well as to some of the papers and works of interest which have been presented since the date of the author's preface." These notes and references are quite numerous, appear to be selected with care and discrimination, and cannot fail, we think, to add materially to the practical value of the book. They are concise, clear and to the purpose, and are so arranged as to add very slightly to the bulk of the volume, while they bring it very

nearly up to the present date, and entirely up to that of its American preface.

We observe, in looking through the chapters, that several of the notes of the previous American editions have been incorporated and acknowledged by the author. This is a courtesy which, although natural enough, must be all the more gratifying and creditable to both parties, because it is not by any means the constant practice of some other equally prominent British authors, who at least appear to have accepted the hints of their American sponsors without seeming to be aware of any obligation. It is true that acknowledgements of this kind are more necessary and more likely to occur in a work that is so entirely made up of matters of fact gathered from every reliable source as a treatise on medical jurisprudence; still, we cannot help thinking our author's example in this instance worthy of note, as one which, in spite of the character of some of our volunteer editing, might, in all legitimate cases, be much more generally followed.

With regard to the improvements introduced by Dr. Taylor, we cannot serve our readers and the interests of the work better than by extracting from his preface, which he calls a "summary of the more important additions."

"Under Poisoning, numerous cases have been added, including new facts regarding the fatal doses of some of these agents, and the pathological changes which they produce. The subject of chronic poisoning has been more fully treated, and various improvements in the mode of applying tests for the detection of poisons have been introduced. In the chapters on Prussic Acid, Morphia, Strychnia, and Aconite, the reader will find some new cases which add to our knowledge of, and amend our experience on the operation of these poisons.

"Under Wounds, the following subjects have received additional illustration:—Ecchymosis,—the production of wounds by falls, and the method of distinguishing accident from homicide,—the influences of articles of clothing in modifying the appearance of personal injuries,—the direction of wounds as furnishing evidence of their origin,—the microscopical and chemical examination of clothes and weapons, especially in reference to the detection of blood, and the means which at present exist for distinguishing human from animal blood—the concealed causes of tetanus,—cicatrices from disease or wounds,—survivorship under severe wounds of the heart and injuries of the head,—ruptures of the liver, lungs, and bladder,—wounds from firearms as furnishing evidence of homicide,—the burning of the human body after death, and remarks on its alleged spontaneous combustion.

"Under Infanticide new cases will be found, which illustrate the causes

of death in new-born children,—remarks on the medical evidence of the survivorship of the child, and the distinction between accidental and homicidal violence.

“The chapters on Pregnancy and Abortion have also received additions; and under Legitimacy, the medical evidence respecting gestation has undergone a full revision, by the aid of recently published facts and observations. It is unnecessary to specify the numerous additions which have been made to the remaining portions of the work. The chapters on Drowning, Hanging, Strangulation, and Suffocation, contain many additional facts, which may probably have an important bearing on medical evidence in future cases. Under Insanity, the provisions of the recent Acts of Parliament regarding medical certificates for the confinement of the insane, are fully described; and some further observations have been made in reference to the plea of insanity in criminal cases. Notices of many important trials (involving medico-legal questions) which have occurred in the United Kingdom from the date of the publication of the previous edition to the Lent Assizes of the present year, have been inserted in those parts of the volume to which the cases specially refer. In short, it has been the desire of the author throughout the whole of the work, to keep it up to the level of the day in regard to medico-legal information.

“In the preparation of this edition, the author has had the valuable assistance of the Right Honorable, the Lord Justice Clerk of Scotland. His Lordship has revised the whole of the sheets, and has not only furnished the author with many useful suggestions for the improvement of the work, but has enabled him to correct some errors which had crept into the reports of cases published in the earlier editions.”

The student who is already familiar with the former edition, will be struck with many minor, but still useful changes, not enumerated in the author's preface. He will find also, that the index has been extended and rendered more complete, both as to topics and cases, and that the whole volume has been very carefully and neatly printed, and that, although presenting one hundred octavo pages of new matter, the work still retains its originally convenient form and size; that in short, it is more than ever an admirable specimen of a compact and comprehensive manual, which, although pretending only to the consideration of a hand-book and affording the facilities for one, yet possesses the weight and accuracy, if not all the fulness, of an authoritative treatise.

As such, we recommend it heartily to every one who may be interested,—and who, in these days of poisoning developements, is not?—in the fascinating details and discussions which belong to its prolific subject.

Osteological Memoirs. No. 1. The Clavicle. By JOHN STRUTHERS, M. D., Fellow of the Royal College of Surgeons of Edinburgh, Lecturer on Anatomy. Edinburgh: Sutherland & Knox. London: Simpkin, Marshall & Co. 1855.

Many of our readers will be surprised to learn that ninety pages have been devoted, by the author of the above memoir, to the description of a single bone, especially when we tell them that the consideration of the subject does not include its relations to pathology or comparative anatomy.

We are certain that most persons could exhaust their knowledge of the subject in a few paragraphs, and would hardly expect to find more than a few pages occupied with the peculiarity of one bone, even in a full text-book on Anatomy. How then, can there be in that single bone enough to interest even the author himself, is a question which might be readily asked.

An accomplished and thorough anatomist looks at a bone, not merely with reference to the general features by which it is recognised as a clavicle or sternum, but in it he sees peculiarities distinguishing it from other clavicles or other scapulæ. To him the ordinary description would be as barren as if you were to describe to a painter or ethnologist the figure of a man by merely indicating his general height, and the enumeration of his limbs, eyes, &c. To the anatomist every bone has its peculiarities. No two clavicles are precisely alike. As human countenances are varied to an unlimited extent, and yet possess the same features, so a bone to an anatomist presents, not merely a rough outline by its form, so that its position in the skeleton may be recognized, but to him it has also an expression peculiar to itself, if we may so speak, and which may even shadow forth the age, sex, strength and carriage of the living individual.

Mr. Struthers' descriptions are entirely from nature, and the minuteness of his detail, the carefulness of his measurements, and the fulness of his observation, show a laboriousness of research and patient investigation which must ever place him in an enviable position among his fellow anatomists.

He truly observes in the Preface:—

“Descriptive anatomy seems to be regarded by many as an exhausted science, but is far from being so. What science is, or can be so,

when nature is the study and man the student? As Bacon has well said, 'They who confidently or magisterially pronounce of nature, as of a thing already discovered, have highly injured philosophy and the sciences.' Each new discovery, or method, or addition, opens the way to farther research and thought, and each new and greater application brings out new facts and principles, which gradually unfold themselves under the patient exercise of observation and thought, the combined use of the bodily and mental eye. I do not write thus of anatomy from enthusiasm; ten years in a dissecting room and at the lecture table afford ample time and occasion for mere enthusiasm to cool; but the longer I teach it, and the more I look into it, the more do I find it a rich field inviting new and farther investigation."

We can hardly expect the general reader, or a practitioner, to take much interest in the above memoir; but to those who are engaged in teaching anatomy or studying it as a science, we can recommend it as a valuable addition to their libraries.

Physician's Tabulated Diary; designed to Facilitate the Study of Disease at the Bedside. By A PHYSICIAN OF VIRGINIA. Richmond: J. W. Randolph. 1856.

The few efforts made as yet, to supply the want indicated by the above title-page, have hardly overcome all the difficulties it presents. This one, perhaps, succeeds as well as any other. It has the advantage, so far as real availability is concerned, in the brevity of its list of items to be recorded, and the consequent convenience of its bulk. There is, nevertheless, a corresponding disadvantage, greater, we think, than it need have been in this case, in the contracted space left for the record of most important particulars, *e. g.*, the symptoms and signs connected with the respiratory and circulatory apparatus, &c. We cannot but commend, however, the highly important purpose of its preparation, as stated to be, "the accumulation, at the bedside, of materials for analysis and generalization, which may, in time, elucidate questions of Medical Topography, Etiology, Therapeutics and Pathology."

THE MEDICAL EXAMINER.

PHILADELPHIA, SEPTEMBER, 1856.

NEW YORK AND PHILADELPHIA.—An article with the above caption appears in the July number of the American Medical Monthly, of New York, under the head of editorial and miscellaneous, which, for the credit of said Journal, we rather prefer to consider miscellaneous than editorial. It smacks altogether too much of provincialism to be the performance of a genuine Knickerbocker, and we shall require better authority than its own intrinsic merits to convince us that it is the production of any one entitled to represent the local “feelings” and interests which it would lead us to regard as so cruelly outraged. We do not consider ourselves, however, as at all called upon to defend Dr. Neill, the Publication Committee, our Medical Schools, Grinders, Publishers, and we can’t remember how many more, who are so unfortunate as to be the subjects of this absurd attack. We may remark, however, *en passant*, that the unworthy attempt to create the impression that the disturbance which so unexpectedly grew out of the resolution offered at Detroit arose from a feeling of jealousy against New York, is so preposterous, that we can hardly think it will impose upon any one,—particularly when it is known that Dr. N. had not consulted with a single one of his colleagues before offering his resolution, although he *had* conferred with some of the most distinguished members of the New York delegation, who fully concurred with him in the propriety of the movement. Our object, however, in noticing the article, was not to defend Philadelphia against the aspersions made upon her, nor the resolution, which speaks for itself, but to ask the attention of our readers to a few remarks upon the following extract. It reads thus:—

“If New York demonstrates to the satisfaction of the stupidest, that it is possible to publish the Transactions better, and more economically by between five and ten hundred dollars a year, Philadelphia clutches back the right to publish, though it does not dare to ignore the lesson.”

Any intelligent reader might have predicted that no verification would accompany the above assertion. Had the writer restricted himself to the simple statement that printing is done more cheaply in New York than in Philadelphia, we should have contented ourselves with the remark that the reverse is the truth, and left the demonstration of the fact to

those who should care to enter upon it. Had he affirmed that New York does better printing than Philadelphia, we should have submitted the 7th and 8th volumes of the Transactions of the American Medical Association to our readers, and confidently abided the judgment of the profession. But when we are quietly assured that our National Association would have saved "five or ten hundred dollars" by printing its Transactions in New York instead of Philadelphia, simple affirmation should give place to demonstration. This demonstration we are happily enabled to furnish, in such a way that our readers must be careless indeed should they fail to understand it.

Let the reader compare the 7th and 8th volumes of the Transactions of our National Association. We do not think it is partiality which induces us to assign a preference for the latter, so far as respects the paper on which it is printed, or the beauty and accuracy of its mechanical execution.

The bill rendered for printing the 7th Vol.	was	-	-	-	\$1372	61
do.	do.	for 8th Vol.	-	-	1620	79
Difference						248 18

Here is an apparent difference against Philadelphia. But, if the reader will carefully follow us, he will see that this difference will eventually appear against New York. The degree in which tabular matter exceeds the cost of plain matter is well known to all who have the slightest familiarity with the press. Now, let the reader cast his eye over the pages of Vol. 8, and he will at once notice an unusually heavy quantity of tabular matter—121 pages; while in Vol. 7 there are but 15 pages of such matter. Now, tabular matter (rule and figure work) not only costs double that of plain matter, but the cost of such matter is additionally enhanced by the fact that it is usually set in smaller type than the text of a given work. Let us assume that an ordinary page of the Transactions costs 75 cents, now, a page of brier rule and figure work will cost \$2 50. The number of tabular pages in Vol. 8 exceeds those in Vol. 7 by 106, which, at \$1 75 per page, would give us \$185 50, reducing thus, by that amount, the difference between New York and Philadelphia bill. Any reader can at once appreciate such distinctions as these. But we rely on very different data to make the question in dispute intelligible to him.

The cost of setting the type for Vol. 7,	-	-	-	-	\$514	41
do. do. do. 8, is	-	-	-	-	822	51 15
Difference						308 10

307.74

Thus, it cost *three hundred and seven dollars and seventy-four cents* more to set the type for Vol. 8 than for Vol. 7. Deduct from this amount the difference already alluded to, of New York bill against Philadelphia bill, and we have a difference against New York of \$59 56. But this is not all. *Eleven hundred* copies of Vol. 8 were printed; while of Vol. 7 there were but 1000 copies printed. 100 extra copies on 48 forms will require $5\frac{1}{2}$ reams of paper, which, at nine dollars per ream, cost forty-eight dollars, increasing by that amount the difference against the New York bill. Not only this; but Vol. 8 exceeds Vol. 7 by *ninety-six* pages, to print 1000 copies of which, required $6\frac{2}{3}$ reams, which, at nine dollars, augments the difference against New York by sixty dollars more. If we add to these items 48 half tokens of press-work on 48 forms, and 24 tokens on ninety-six pages, in all, 48 tokens a 40 cts., we shall find the difference against New York still further increased by \$19 20; total balance against New York, *one hundred and eighty-seven dollars and thirty-six cents.* (\$187 36.)

As regards the first six volumes issued in Philadelphia, the cost of printing, we may observe, was considerably less than that of Vol. 8.

We have thus shown that Philadelphia printed the *Transactions* 10 per cent. more cheaply than New York. How much better, the profession must determine.

MEDICAL NEWS.

THE New York Journal of Medicine and the New York Medical Times have been combined, under the title of the New York Journal of Medicine. The Editors are Dr. S. S. Purple, S. Smith and H. D. Bulkley. The Times was an excellent and ever welcome Journal, and we shall greatly miss its disappearance from our list of exchanges.

MEDICAL DEPARTMENT OF PENNSYLVANIA COLLEGE.—The following resolutions, in relation to the resignation of Prof. Allen, were unanimously adopted by the Faculty, and ordered to be published:—

Resolved, That the Faculty of the Medical Department of Pennsylvania College sincerely regret that their colleague, Dr. J. M. Allen, has felt constrained to resign the Chair of Anatomy, which, for several years, he has filled with signal ability and distinction.

Resolved, That in accepting Dr. Allen's resignation, the Faculty desire to assure him of their high estimate of his professional talent, to reciprocate his expressions of friendly feeling, and to offer him their cordial good wishes for his success in whatever career he may determine to pursue.

FRANCIS G. SMITH, M. D.,

Registrar pro. tem.

A disciple of Dr. Coffin has been tried at Lincoln, and sentenced to three months' imprisonment, for manslaughter by the use of *Lobelia inflata*. The victim in this case was the wife of a laboring man, and was said to be suffering from fever, but was in a convalescent state; when the quack doctor introduced himself to her, he pronounced that she had "abscess in her inside," and that he would cure her. He accordingly administered the panacea, which was taken so regularly and so effectually, that, after great suffering and delirium, the poor woman died. The cases of poisoning by *lobelia inflata* have been so numerous of late years, that this drug ought certainly to be classed among the prohibited articles, if any legislation should take place upon the sale of poisons. In the case to which we now allude, the punishment of imprisonment for three months appears a very light one; but it has been so frequently the case that prisoners charged with poisoning by *lobelia inflata* have been triumphantly acquitted (sometimes under the direction of the presiding judge), that the mere fact of a conviction is a subject for congratulation.—*London Med. Times and Gaz.*, July, 1856.

In a former communication I stated my wish to bring under your notice the fatal case of small-pox, the result of inoculation with variolous matter, which occurred some time since in the neighborhood of Ballinasloe. It is lamentable to see the obstinacy with which the lower classes adhere to this most baneful practice, and their blindness in allowing themselves to be made the dupes, and their children the victims, of the ignorant and unprincipled individuals who infest the country in pursuit of their unholy traffic. It is also humiliating to observe how little effect legislative enactment has in checking this monstrous evil, chiefly in consequence of the great unwillingness of the people to give evidence against the delinquents. An eminent practitioner writes, "Small-pox inoculations have been extensively practised in this town and neighborhood. Though we had sufficient moral evidence of this, we were not able to prove its legality in any case previously to those referred to in the newspapers. The culprit in the late instance had, on pleading guilty of one offence, been sentenced to a fortnight's imprisonment, and, subsequently to the sentence, information was received of a case which had terminated fatally. The resident magistrate, with laudable determination, insisted on a post-mortem examination being held, and the coroner's inquest, which had been summoned, was adjourned until the autopsy should be performed. The result was a verdict of manslaughter. The consequences of this practice of inoculation have been very disastrous. Variola rages in this town with great violence, in some instances attacking the upper classes of society, and I trust that

something useful may result from the verdict which has been obtained." The evil described in the foregoing communication has evidently attained a fearful height, and loudly calls for effective interference. It appears to me that the statute bearing on the point is far too lenient; I can find in force in Ireland on the subject, only the 3d and 4th Victoria, c. 29, § 8, which provides that persons inoculating, or otherwise producing small-pox, shall be liable to be proceeded against at petty sessions, and, on summary conviction before one or two magistrates, may be imprisoned for a period not exceeding one month.—*Dublin Correspondent of London Med. Times and Gaz.*

DR. VALENTINE MOTT.—In an interesting paper by Dr. Stewart in this month's *Edinburgh Journal*, describing the condition of the Medical Profession in the United States, he thus enumerates the successful career pursued by Dr. Valentine Mott as an operative surgeon:—"The whole world accords him the credit due to his first and greatest undertaking—the ligature of the arteria innominata. The patient, whose primitive iliac he tied more than twenty-five years ago, still lives in the enjoyment of perfect health. He has ligatured the external iliac artery seven times, and lost three of the patients; the internal twice, both of whom were cured; the femoral artery fifty-two times, with the loss of one patient; the primitive carotid forty-two times, with a fatal result in two instances; the subclavian, *without* the scaleni, six times, all successful; and *within* once, the patient dying of secondary hæmorrhage. His original operations upon the maxillary bones have been very numerous; and he considers his exsection of the clavicle for osteo-sarcoma as the most difficult and dangerous operation that can be undertaken. He has cut for the stone 164 times, with a loss of seven patients. His operative career, though he is nearly 70 years of age, has not yet terminated, for he writes word that his second case of ligature of the internal iliac has just left the city."—*Med. Times and Gaz.*

DEATHS IN THE FRENCH ARMY.—The French Government has just published an account of the number of deaths that occurred in the Army of the East between May 1, 1854, when the embarkation for Turkey commenced, to March 30, 1856, when the Treaty of Peace was signed:

Officers of all ranks,	-	-	-	-	-	1284
Non-commissioned officers,	-	-	-	-	-	4403
Soldiers,	-	-	-	-	-	56,805
Total -						62,492

The following are the returns of deaths of all ranks and arms which

occurred during the above two years in the entire French army, not in the Crimea :—

In Algeria, - - - - -	5246
In Italy, - - - - -	1088
Baltic Expedition, - - - - -	1059
In France, - - - - -	13,636
<hr/>	
Total - - - - -	21,028

As the army in the Crimea was never more than about a fifth of the entire French army, the mortality during the Crimean campaign has been indeed enormous. Many single regiments lost more than 1000 men. Thus, the 7th Infantry lost 1662; the 26th, 1511; the 28th, 1505; the 98th, 1471, etc.—[We have reason to believe that the mortality was in reality much greater.—*Ed. London Med. Times and Gaz.*]

THE FRENCH HOSPITALS IN THE EAST.—The great hospital at Pera has recently been closed, owing to the return to France of a portion of the French army. There were at one time as many as 2000 patients in this hospital, and no less than 27,500 were received during the two months it remained open. Out of this number, 13,000 were sent to France or to other hospitals in a convalescent state. The *Gazette Médicale de Paris*, in recording these figures, does not say whether the remaining 14,500 died; such, however, might perhaps be the inference. The total number of days' treatment was 633,985.—*London Lancet.*

In the March number, 1856, of the *Examiner*, we published a translation of a paper, entitled, "Results of some Statistical and Physiological Researches on Twins;" at the end of which, the Translator added some remarks, giving the results of *his own* investigations of the subject gathered from the published statistics of the Dublin Lying-in Hospital. In the July number of the *Glasgow Medical Journal*, we find the *whole* article copied, not only without the customary credit, but with the following introductory:—"At a meeting of the French Academy of Sciences, November 25, Mr. Baillarger read a paper containing the results of some Statistical and Physiological Researches on Twins. We have thought the subject presents some features which might interest the readers of the *Journal*." Precisely the same words introduced the paper in our own *Journal*, excepting that the word "*Examiner*" is used in place of the "*Journal*." From the change of word, it would almost seem as if "*the Journal*" claimed the translation of the paper for itself. We are willing to believe, however, that the omission of the usual courtesy was an oversight, and we take this opportunity of drawing the Editor's attention to it.

We not unfrequently notice our own articles thus appropriated by other *Journals*. A little more attention to the rules of courtesy and the rights of others would prevent such mistakes, which are disagreeable to both parties.

RECORD OF MEDICAL SCIENCE.

Symptoms of diseased Elbow-Joint—Intense Pain—Operation—Abscess in the Olecranon, with Necrosed Bone—Recovery. Under the care of Mr. CURLING.

Benjamin Smith, aged 33, by trade an engineer, of steady habits, and usually enjoying good health, was admitted into the London Hospital, on March 29, 1853. The particulars of his affection, both in its history and subsequent progress, were carefully recorded by Mr. John Langston, one of Mr. Curling's dressers, of whose notes the following is an abstract:—

March 29.—He applies on account of what looks like disease of the left elbow-joint. There is some swelling around the whole joint, but it is chiefly limited to the posterior part, where the integuments are tense and red. The arm is held rigidly flexed at a right angle, and there is exquisite tenderness about the part, more especially from the olecranon, down the course of the ulna, the slightest touch on which causes him to flinch. He states that for some weeks past he has had most severe and constant pain in this part, and his haggard appearance fully corroborates his account. He states that he was in usual health up to January 15, and had but recently returned from a voyage to the Cape of Good Hope. On the day mentioned, he was attacked by what seemed to be acute rheumatism which confined him to bed for more than a week, and during which several of the joints were much swollen. The general symptoms subsided under treatment, and with them the pain in all the joints, excepting the left elbow, in which it steadily increased. At first there was neither swelling nor redness about this joint, but only an intolerable aching pain. For a week or two before admission, he had attended as an out-patient, under the care of Mr. Ward, and iodine paint had been applied to the part. There were two circumstances in the account of his previous illness which were of importance to be borne in mind in connexion with his present affection, viz., that he had on two occasions prior to the one just mentioned suffered from acute rheumatism, and that at the age of 2, he had, in consequence of a severe blow, had an attack of severe inflammation about the same elbow as that now affected. This attack had been sufficiently urgent to necessitate his attendance for some time as an out-patient at a Hospital. Leeches and blisters were applied, and an abscess opened, but eventually he got quite well, and had perfect use of the joint. From the state of the parts and the character of the pain, Mr. Curling inferred that the disease was rather in the bone itself than the joints; as no movements or manipulation could, however, be borne, the diagnosis was only conjectural.

Ordered six leeches, followed by poultice. Low diet.

March 31.—The pain continues as severe as ever, but with some paroxysmal augmentations of severity. It almost entirely prevents

sleep. He is to have half-a-grain of the acetate of morphia every night at bed-time.

April 3.—No relief. He has lost appetite, and is rapidly losing flesh and strength. It now appears that, prior to admission, he had of his own accord taken large doses of laudanum for the relief of the pain. Under the influence of chloroform, Mr. Curling this morning made a deep puncture over the olecranon, but no pus was obtained. It was found that the joint had perfect mobility while the patient was unconscious, and no signs of disease within the articulation could be discovered. Two leeches were to be applied every other night, and the dose of morphia increased to a grain.

11th.—The severity of the pain and its nocturnal exacerbations have so far enfeebled the patient that he is now scarcely able to stand without assistance. Nothing whatever has been gained by the treatment. He is to discontinue the leeches, and have a strong opiate lotion applied to the part.

16th.—A blister over the external condyle is to be applied. Morphia in grain doses every six hours.

20th.—It has been determined in consultation to make more free incisions over the ulna.

21st.—This morning Mr. Curling made a free incision along the course of the ulna, from above downwards, for a length of nearly two inches. No pus was obtained. The limb is confined to an angular splint, and the man is to have six ounces of wine a-day. Continue the morphia.

26th.—There has been a slight relief, in some respect, until to-day, when the pain is as severe as ever. The wounds are healing, and do not discharge more pus than their own surfaces supply.

27th.—A general consultation was held on the case to day. Mr. Luke suggests, and Mr. Curling is inclined to agree with him, that there is probably matter pent up within the bone. Others are of opinion that the intense pain depends upon commencing ulceration of the articular cartilages, and advise a course of mercury. As the patient has been a little better during the last week it is decided to wait.

May 5.—The pain has been very intense ever since the last note. The man describes it as like plunging a number of needles into the part. A course of the iodide is to be tried. \mathcal{R} Pot. iodid. gr. iij; dec. sars. \mathfrak{z} i ter die.

12th.—The dose of the iodide is to be increased up to 5 grains.

13th.—Catarrhal symptoms from the iodide have set in with some severity. The dose is to be taken but twice a-day.

14th.—Catarrh more severe. The iodide to be suspended.

25th.—No permanent relief whatever having been obtained by any of the various plans of treatment which have been tried, it has at length been decided to trephine the head of the ulna. The condition of the part is much the same as at the time of admission, excepting that the swelling is more decidedly limited to the back of the joint, and the parts overlying the ulna. The incisions made have wholly healed, excepting one small sinus which leads down to the bone; but from this

there is very little discharge indeed, and the surface of the bone is not bare to any extent.

Operation.—The man having been put under the influence of chloroform, an incision two inches long from near the extremity of the ulna downwards, and a second inwards at right angles with the first, were made. The skin having been dissected up, the bone was laid bare. On examining the part to which the sinus led, a very minute opening was discovered, leading into the bone. This was taken as a guide for the application of the trephine, and a small circle having been moved by the latter, the cavity of an abscess containing some necrosed fragments was opened. From this cavity two portions of dead and perfectly loose bone—one about the size of a nut, and the other that of a horse-bean—were removed. This seemed satisfactory; and, as the cavity very nearly opened into the joint above, no prolonged exploration was made. The elbow was wrapped in wet lint, and the arm confined in an angular splint.

26th.—The man has had excessive pain, and, in spite of increased doses of morphia, has had no sleep.

27th.—No rest has been obtained. The elbow is poulticed.

29th.—The severe pain continues. A drachm dose of laudanum is to be taken every four hours, instead of the morphia.

June 2.—The pain is greatly abated; the patient has had good sleep, and is much improved.

7th.—Rapidly improving in every particular, and now able to walk out in the garden. Wound healthy in appearance, and discharging freely.

15th.—The wound is now nearly healed, and the patient is free from pain. The arm is still, however, kept in the flexed position, though it is quite certain that the joint is not diseased. The man is to leave the Hospital, and go down to the Ramsgate Sea-bathing Infirmary very shortly.

After remaining about five weeks at Ramsgate, during which the wound quite healed, the man was discharged. He had quite regained his health, and was free from pain; the motions of the elbow had, however, not been then perfectly recovered, though there seemed every reason to expect that they shortly would be so. He has not since been seen.—*Medical Times and Gazette.*

Symptoms of Diseased Elbow-Joint—Removal of Necrosed Bone from the Olecranon—Partial Recovery. Under the care of Mr. CURLING.

The following are Mr. Langdale's note of the case:—

Emma Simmons was admitted into Sophia Ward, London Hospital, May 31st, 1853. A young woman of short and robust figure, with light hair, blue eyes, and somewhat ruddy complexion, following the occupation of dressmaker. From the county of Norfolk.

The history of the case shows that about ten years ago, while walking in frosty weather, the ground being very icy, she slipped and fell upon the pavement. The greatest part of the injury sustained on this occasion, was borne by the left elbow, and it in a day or two became much

swollen and painful in the extreme. Slowly an abscess formed, occupying in its progress two or three months; she then applied to a Surgeon in the neighborhood, but not getting any advantage from the treatment had recourse to, after two or three months more, she relinquished it. Subsequent to the successive formation and maturation of two or three other abscesses about the part, which last all perfectly healed again, it assumed a more or less chronic condition. But the first established abscess left a permanent fistulous aperture, near—and leading down to—the subcutaneous head of the ulna, through which a puriform discharge constantly escaped to the amount of about an ounce per diem. It was not particularly painful during this stage, aching occasionally, and in damp or wet weather, using the patient's own phrase, it was especially "pricking and shooting." For eight years in a somewhat similar state, the arm was in use, in the performance of the duties of a domestic servant, there being no rigidity, and occasionally the discharge ceased for a period. During the past year, however, it again became hot, painful, and swollen. The aching again became intense, and felt as if it arose deep within the bone—was worse at night—prevented rest, and the arm assumed a rigid flexed position. The patient found most alleviation from the use of cold water, constantly applied. The attendant constitutional symptoms, were loss of appetite, feverishness, &c., and excessive debility.

Nevertheless, by resting the arm upon a cushion and leaving the hand free, with the assistance of the sound right arm and hand, (for a week or two at intervals when the symptoms were less severe) she was able to do needlework. It got still worse, and the patient now entered a Hospital, where she remained for three weeks. She states that the limb was proposed for amputation and the patient herself consented; but, through the non-acquiescence of her friends it was not carried into effect, and she left the Hospital in a less favorable position than before.

In about ten days, having procured a ticket of recommendation, she was taken into the London Hospital. After a careful examination of the part, under chloroform, Mr. Curling convinced himself, that in this case, as in the preceding, the disease was limited to the ulna and did not include the joint itself. An operation very similar to that performed in it was accordingly had recourse to, and the trephine having been used, some small fragments of dead bone were removed. The bone was found generally thickened and porous, and it was evident that the disease partook of a strumous character. The patient did remarkably well afterwards. Thenceforward all pain ceased and the thickening gradually subsided. After a while she was sent to the Margate Seabathing Infirmary, where she regained her health well.

When last seen, shortly after her return from Margate, there was still an open sinus which led down to bone. The joint, however, was evidently quite free from disease, and the arm in a comparatively useful condition. The strumous nature of the disease of the bone was no doubt the explanation of its not taking on a healing action. There was no reason to suspect that any necrosed fragment remained.—*Med. Times and Gazette.*

Post-Mortem Appearances of a Case in which Tracheotomy had been performed three years previously. By HENRY SMITH, Esq.

In the Medical Times and Gazette for July 9, 1853, will be found the particulars of a case in which the operation of tracheotomy was performed for urgent dyspnoea, the result of acute inflammation of the larynx. The patient, who was at that time a fine hearty young man, aged 28, recovered from the effects of the operation, but he was not able to dispense with the tube. This circumstance was, of course, a source of considerable anxiety to himself and his friends, although he was comparatively comfortable, and was enabled to attend to his duties; nevertheless, when he made any unusual exertion, or was exposed to cold, he suffered from difficulty of breathing and irritation; he was, however, enabled to pass away many hours together with the tube corked up, and this circumstance induced me to think that it might be dispensed with.

With a view to obtain sanction for withdrawing the tube, the majority of the most eminent Physicians and Surgeons in London were consulted at various times, and the greater number considered that the patient might be able to breathe through the natural aperture. One distinguished Surgeon, however, with whom I had a consultation on more than one occasion strongly urged, both upon the patient and myself, the impropriety of closing the artificial opening, and this advice, coupled with the nervous timidity of the patient, prevailed; the tube was always retained.

I had lost sight of this patient, professionally speaking, for nearly three years after the operation, when, on the morning of June 30, I was hastily summoned to see him with Dr. Tunaley, who had been called to him a few hours previously, in consequence of an attack of dyspnoea with which he had been somewhat suddenly seized, and which was dependant, not upon any mechanical obstruction to the tube, but upon a general congestion of the lungs, at first unattended with any secretion, but in a short time accompanied with a profuse discharge from the air passages. Both Dr. Tunaley and Dr. Watson, who had seen him some few days before this attack, had diagnosed a diseased condition of the lung on the left side; this was now well marked. The symptoms under which the patient now labored were most severe—terrible attacks of asphyxia succeeded one another, and he died from exhaustion in forty-eight hours.

In conjunction with Dr. Tunaley I examined the body the next day, and the following were the post-mortem appearances presented:—

The epiglottis was free from disease, but erect, and could with difficulty be made to close over the larynx. The entrance into this portion of the tube was so contracted that a small crow quill would hardly pass it, and on laying it open, great thickening in the submucous texture of the whole of the interior of the larynx was observed, but there was no disease of the mucous membrane, which was at this point smooth and shining, and presenting no cicatrix of previous ulceration.

The artificial opening was just below the cricoid cartilage, and beyond this the whole of the inner surface of the trachea was not only most intensely inflamed, but at several points it was deeply ulcerated; the in-

flammation extended to below the bifurcation, and the bronchial tubes were much dilated.

Both lungs were greatly congested, but in addition, the upper and back of the left was rendered almost solid, a great number of tubercles being scattered here and there. On the right side there was less disease, but in the middle lobe a large mass of solid tubercular tissue was observed.

Independent of its intrinsic features of interest, this case is worthy of record, for it was well known to the majority of the more eminent Physicians and Surgeons in London, who were consulted by the patient on various occasions, and some of whom will doubtless recognize it, and will be interested to know the sequel, and the pathological condition of the parts, respecting which there was much discussion during life.

The examination fully reveals the fact, that had this patient survived, the tube must have been worn for life, and yet nearly every one consulted on this point, either recommended that the artificial opening should be closed, or held out strong hopes to the patient that he would some time or another get rid of the tube, and he was only prevented from making the attempt by the opinion of one distinguished Surgeon who strongly insisted upon the danger of such a proceeding. That this advice was correct, the condition of the entrance to the larynx fully demonstrated, still those able men who gave a contrary opinion had good grounds for doing so, for the patient could remain many hours when tranquil, breathing with the tube stopped up. And as during the last few months of the patient's life it was evident that mischief was being set up in the lungs, it was thought that the continued presence of the tube might have caused this. I must confess, that after having had opportunities of seeing the state of the patient under various conditions, and when he was breathing both with and without the use of the artificial opening, I was strongly inclined to agree with the majority of those who urged the patient to remove the tube. Such a case as this will probably not often be met with; under similar circumstances, however, there would always be a difficulty to decide whether the patient might or might not safely dispense with the tube, and it appears to me that the particulars of the case may serve to throw some light upon this point.

There is one circumstance connected with the case which must not be omitted. In the Spring of 1855, Dr. Theophilus Thompson saw this patient, and after making a most thorough examination, diagnosed commencing tubercular deposition in the left lung; moreover, he made the following note with reference to the condition of the windpipe, and with which he was kind enough to furnish me:—

“Very little air seems to pass the glottis; there is a granular state of the fauces; a similar condition in the larynx might explain the reduced passage of air, *except that the sound conveys the idea of simple narrowing, rather than unevenness.*”

The post-mortem appearances show how correct was Dr. Thompson's view.—*Medical Times and Gazette.*

An Account of the Arrangement of the Muscular Substance in the Urinary and Certain of the Generative Organs of the Human Body.
By GEORGE VINER ELLIS, ESQ., F. R. C. S., Professor of Anatomy in University College, London.

It would be vain to attempt to represent this important communication by an abstract; all that can be done is to draw attention to some of the most striking points in it. After a minute description of the three more or less perfect strata of involuntary muscular fibres which constitute the muscular substance of the bladder—viz., an external or longitudinal, a middle or circular, and an internal longitudinal or submucous, stratum—the author proceeds to trace them through the rest of the genito-urinary apparatus. With respect to the prostate, after a minute description of its structure, the author deduces that it is less of a glandular than of a muscular body, and is only a largely developed portion of the circular muscular layer that invests all the urethra behind the bulb or spongy portion, and which is continuous, without interruption, with the circular fibres of the bladder. As the prostatic enlargement includes only part of that muscular stratum of the urethra, the author proposes the name *orbicularis, vel sphincter urethræ*, for both the prostate and the prolongation around the membranous portion of the urethra; while he would confine the old term *prostate* (without the word *gland*) to the thickened and more powerful part near the neck of the bladder. The submucous layer of the bladder is traced throughout the whole length of the urethra. A muscular covering of the vesiculæ and vasa deferentia, consisting of two layers of fibres, (one longitudinal, the other transverse,) is next described; and the paper concludes with a very elaborate description of the sheaths surrounding the spongy structure of the penis.

Mr. Quain was very glad that Mr. Ellis, of whose accuracy the Profession might be assured, had given so good a description (which had long been wanted) of the muscular structure of the prostate gland, and he should be glad to see, from the same pen, or from some other, an account of the glandular structure.—*London Med. Times and Gazette.*

Extraction of Needles, &c., from the Human Body. By ELISHA P. FEARING, M. D., of Nantucket.

In July, 1855, I was called to a single woman, aged 44 years. She was quite fleshy. She complained of great pain and tenderness in the lower part of the abdomen. The lady with whom she had resided for the last fifteen years, informed me that she was insane about ten years ago, and a part of the time in close confinement, but since then was thought to have been rational; that for several years past she had had frequent turns of vomiting a substance about as thick as paint, of a chocolate color (in the opinion of the medical attendant, not blood,) and large quantities of bloody pus and other matters; that she had discharged something of the kind *per anum*, and that not long before I was called she had discharged, and had taken, from the rectum *several pounds of a light mahogany-colored substance, in masses or lumps, of*

various forms and sizes; also, that about the same time she was troubled with a hard and rather painful swelling in the region of the sigmoid flexure, attended with a sense of weight and dragging down, especially when lying on the opposite side; and that the swelling and pain had gradually subsided as the lumps were discharged.

Believing there were more of these lumps, I prescribed an active cathartic. The first discharges were feculent, followed by more of the same kind of lumps; these last completely filled up the rectum, and caused great suffering. It was with difficulty I removed the largest, which measured *six inches in circumference*. Upon the surface of them was some mucus, and lard, which was used to facilitate their removal. They had evidently been permeated by an oil. Nothing of the kind has since been discharged. A specimen has been analyzed by C. T. Jackson, M. D., State Assayer, and the result at which he arrived is, that "this mass of matter is *dried brown ochre paint*." He "detected linseed oil."

A few days after the clearing out of the paint (so called), I removed a common sewing needle, about an inch below the ensiform process. She has since suffered much from nervous irritation caused by numerous needles, as the event has proved. For, during the last twelve months, *one hundred and twenty-three needles, twelve halves or fractions of needles, and two headless pins, have been extracted*. A large proportion of them were taken out from the region of the stomach and abdomen, following in the track of the colon, and many in the immediate neighborhood of the sigmoid flexure. They have also been extracted from other parts of the body and a small number from the limbs; viz., neck, just below the breast, back, loins, just below the left hip, upper and inner part of the thigh, labia pudendi, urethra, perinæum and sphincter ani. One only has been removed from below the knee. One of the largest needles was removed this day, from just below the navel; it has troubled her for a long time, and was situated deeply and obliquely, with the point towards the surface, the point having been bent in the extraction. Some few more can be just felt, but they are too deep to be extracted at present.

The needles varied much in size, and were found in different positions; many were perpendicular to the axes of the body, with points presenting—others more or less oblique—some with eyes broken, some with points broken, and a few without either points or eyes. Probably some were broken off when extracted, as were some of the needles. A few of them have undergone little or no change; but by far the largest number are slightly oxidized, having lost their brightness and become brittle; others are more or less corroded. This difference may be owing to the degree of purity of the metal and their locality.

The motion of the needles, no doubt, depended very much upon their situation, and the action of the muscles; for strong muscular action (no matter from what cause) was almost sure to bring forward a crop of needles—that is, force them nearer to the surface, some very near, while others were scarcely perceptible, requiring a rather tedious and

painful operation. Eight is the largest number extracted in one day, or at one visit.

A question naturally arises, whether the needles and paint were swallowed, or introduced from without? This question cannot be satisfactorily solved, for I have not been able to obtain information affording the least light in regard to this subject, and must therefore rely upon circumstances and facts as they have become manifest.

Not the least mark or trace of a needle, or any other thing, having been forced through the skin from without, has been discovered, notwithstanding the frequent minute examinations made for that purpose; and within twenty-four hours after such examinations, several needles have been taken out from the part of the body examined, having the same blackened appearance. Had they, a few days previous, been forced through the skin, it would seem almost impossible that the trick should have escaped detection. There are other facts and circumstances which would seem to sustain both sides of this question, but for the want of time and space they must be omitted.

Taking all things into the account, I am inclined to the belief that she did swallow, at least, a portion of the needles (probably in papers,) and large quantities of the paint, and did introduce some from without, and, in fact, stuff herself not only with these articles, but with some small pebbles of quartz, which had been taken from the vagina before I saw her, and with whatever else came to hand. Whether sane or not at the time, is not known, in all probability not; for of all things needles would be about the last any sane person would think of swallowing, or forcing in from without in such numbers. It has occurred to me, whether, if the needles were swallowed, they might not have remained a long time in the paint, and thus been preserved, or protected from the corrosive effect of surrounding substances. I have recently seen a part of a needle, which was taken from the forefinger, nearly six months after it was accidentally forced in. It had not changed at all, except having lost its brightness. I have scarcely a doubt that they may, under some circumstances, remain in the body for a long time, perhaps for years, without much change by oxidation. In regard to this very extraordinary case of needles, I will, in conclusion, just observe that, no doubt, a very small number of needles remain to be extracted, which are not accessible at present.

The needles and a specimen of the paint have been deposited in the Cabinet of the Boston Society for Medical Improvement.—*Boston Medical and Surgical Journal.*

Landolfi's Treatment of Cancer.

M. Landolfi's mode of treating cancer having gained considerable notoriety in Austria, he repaired some time since to Paris, in order to induce the Surgeons of that capital to endorse the favorable opinions expressed by some of the Vienna practitioners. The French Hospital Surgeons accordingly appointed a committee of their body to examine into the stability of the claim, and this was done by assigning M. Lan-

dolfi a certain number of patients at the Salpêtrière. The committee, after watching the results of his treatment of these cases, has just made its report, and the following are the conclusions arrived at. From these it would seem that the remedy is destined to fall into the oblivion that has engulfed so many of its predecessors.

1. M. Landolfi's method is made up of both local and internal treatment. 2. The latter, which consists in the administration of chloride of bromine, does not possess the slightest special therapeutical value in the treatment of cancer. 3. The local treatment consists in the application of the following caustic:—Chloride of bromine, 3 parts; chloride of zinc, 2 parts; chloride of antimony, 1 part; liquorice powder, 1 part. 4. Of these substances, the chloride of zinc and chloride of antimony have been long known and employed as caustics. These two chlorides combined in the same proportions as in Canquion's caustic, form the only portion of M. Landolfi's preparation that is really active. 5. The chloride of bromine only acts by raising the epidermis, and exposing the denuded part to the action of the other two chlorides, a result easily obtained by any vesicatory applied just before employing Canquion's paste. 6. M. Landolfi's preparation is, in fact, only this caustic masked by a coloring and odorous body, which, although it leaves the causticity unimpaired, destroys the précision of application. The chloride of bromine has only spoiled the mixture by rendering it fusible, much more difficult to manage, and much more uncertain in its results. 7. As the caustic so modified does not secure the patient from erysipelas or consecutive hæmorrhage, it can be no longer affirmed that its employment is exempt from danger. 8. Infinitely more painful than most others, this caustic induced most severe suffering, which in general lasts for six or eight hours, and may be prolonged for more than twenty-four hours. Opium and other narcotics are powerless against these pains, while their duration forbids our even thinking of employing anæsthetics. 9. The mode of application is quite vicious, and opposed to the rules of art. In place of attempting to at once destroy the cancerous tumor, M. Landolfi attacks it by partial and successive applications—a necessary consequence of employing a caustic the extent of the action of which cannot be calculated. 10. These successive applications, repeated on some patients fifteen or twenty times, induce a total amount of suffering hitherto unheard of. 11. They prolong the treatment indefinitely, and infinitely delay cicatrization. 12. The incessant irritation thus induced is of a nature to favor relapse, as experience has only too well shown, and as all know who are imbued with sound surgical knowledge. 13. This method, applied by the inventor himself to nine cases of cancer of the breast and three cases of cancroïd, has given the following results:—Of the 9 cases of cancer of the breast:—2 have died, 4 have suffered a notable aggravation of the disease, while in three cases in which cicatrization took place, the disease immediately after re-appeared; that is to say, in no case did a cure result. Of the 3 cases of cancroïd, a cure took place in 1; in another there was cicatrization with re-appearance of the disease, and in the

other an exacerbation took place that necessitated the amputation of the limb.

To sum up, M. Landolfi's method can only be applied to certain cancers; it is more painful and more uncertain than several other modes of cauterisation; and it is, in particular, inferior to Canquion's method, of which it is only an altered copy. Like all the other methods of treatment, it may succeed in destroying certain tumors and cicatrization may follow; but it is quite powerless for the prevention of relapse, which it would seem rather to provoke, and so far from forming a step in advance, it adds but another to the illusions that so abound in the history of cancer.—*Med. Times and Gaz., from Bull. de Thérap.*

Treatment of Nocturnal Erections. By M. DEBOUT.

When these occur during the course of a gonorrhœa, or are produced by genital erethism in the young, M. Van den Corput recommends the following pills:—Extr. of belladon. 10 centigr., fresh lupulin, camphor, aa 60 centig. Divide into eight pills, from one to four to be taken towards evening. The saccharate of lupulin always succeeds, when given in sufficient doses, in the relief of the erections, which are sympathetic of specific inflammation of the mucous membrane. A better combination than that of camphor and belladonna is formed with indian hemp:—Extract of indian hemp 5 centigrammes, recent lupulin 1½ grammes, sugar 9 s., for the trituration of the lupulin. To form two doses, to be taken in the evening at an hour's interval. As to the erethism of children, it is usually induced by ascarides in the rectum, and may be relieved by injections of cold water or of salt and water.—*Ibid from Ibid.*

Effects of Digitalis on Generative Organs.—Mr. Brughmanns says, that if from 35 to 50 centigrammes of pulv. digitalis be given for five or six days, the most complete hyposthenizing effect is produced on the generative organs. He has thus given it with very great advantage to combat erotic excitement, whether due to excitable temperament, sedentary life, stimulant regimen, or the privation or excess of venereal pleasure, etc. He also finds it very useful in subduing the inflammatory accidents that so often accompany syphilitic diseases, and which may be prevented by its early administration. It is pre-eminently useful when phymosis or paraphymosis, chordee, epididymitis, or adenitis are either present or feared.—*Med. Times and Gaz., from Rev. Méd. Chir.*

PARIS, July 22d, 1856.

The striking differences which seem to exist between the genital organs of males and those of females in the superior classes of animals, and the differences which seem also to exist between the products of the secretion of the ovaries and that of the testicles, are merely appearances, and not true differences, according to two papers which have just been published here. In one of them, Professor Serres tries to prove

that the series of metamorphoses which takes place in the seminal fluid, takes place also in the product of secretion of the ovaries. In the mother-cells of the semen, M. Serres finds the three elements of the primitive ovum, *i. e.*—1. An outside vesicle (the mother-cell of previous anatomists); 2. A small enclosed cell, corresponding with the germinal vesicle of the ovum; and, 3. A nucleus in this small cell, corresponding with the germinal spot of the ovum. The duplication of the enclosed cell in the semen takes place in the same manner as that of the germinal vesicle. In the semen, the cleaving of the cells is spontaneous; but, in the ovum, the cleaving of the germinal vesicle is not spontaneous, and takes place only after fecundation.

In a paper, communicated last week to the Société de Biologie, M. Charles Rouget, one of the most distinguished of the young French anatomists, has begun the exposition of researches upon the analysis between the male and the female genital organs. The object of this first paper is to show that the round ligament corresponds exactly with the gubernaculum testis. In point of structure, these two organs are alike; they are composed of blood-vessels and nerves coming from the same sources, and we find in them both striated muscular fibres coming from the same muscle. Besides these, there are many unstriped muscular fibres in the two organs. The analogy of these organs is very evident when we compare their mode of development in embryos. M. Rouget grounds his views on this subject upon researches made, not only on man, but on a great many species of mammals.

A communication, made by M. Cloez to the Académie des Sciences, shows that the re-agent employed to detect ozone in the atmosphere may lead to erroneous conclusions. This re-agent, iodole of potassium, may give the same reaction as with ozone, when it is exposed to the evaporation of nitric acid, and to the emanations from resinous trees and aromatic plants.

M. Briquet has read a paper on the elimination of quinine, at the same Academy. He has found that quinine passes very quickly into the urine. In half-an-hour after it has been ingested it is found in the urine; but the complete elimination of a dose requires many days. Urine seem to be the only secretion with which quinine is eliminated.

Dr. Brown-Séquard has shown to the Société de Biologie, that almost all the excretory ducts of glands, in birds, have rhythmical movements. Already Professor Claude Bernard had found that the choledochus and the pancreatic ducts have such movements.

Dr. Brown-Séquard has shown that the same thing exists in the vasa deferentia in adult birds. The movements in all these ducts are perfectly regular, and their rhythm seems to have no constant relation with that of the heart. He has ascertained that the number of contractions of each of these ducts in a given time, is not the same in one of them as in the others. The left vas deferens has not its contractions at the same time as the right. When the spinal cord is destroyed in all its length in a decapitated bird, the rhythmical movements continue for some time in the choledochus, pancreatic, and urinary ducts, and in the vasa deferentia, as well as in the heart. If the heart is taken away, all

these excretory ducts still have, for a few minutes, strong rhythmical contractions. Dr. Brown-Séguard points out also the existence of rhythmical contractions of the trachea and bronchiæ in large sea-birds at each expiration. These contractions, which diminish the calibre of these tubes, contribute to the expulsion of the air.—*Paris Correspondence of the London Medical Times and Gazette.*

Rules for Restoring the Drowned. Drawn up by Dr. MARSHALL HALL, M. D., F. R. S., &c.

The following rules are the *result* of half a year's investigation of Apnoea and Asphyxia—a subject which I propose to prosecute still further, knowing that truth only comes of long continued labour and research. I wish especially to put to the test of careful experiment, the correctness of the dogma, that if the heart has once ceased to beat, its action can never be restored—a dogma calculated to paralyse our efforts in many cases in which hope may really not be *totally* extinct:

1. Treat the patient instantly, on the spot, in the open air, except in severe weather, freely exposing the face, neck, and chest to the breeze.

2. Send with all speed for medical aid, and for articles of clothing, blankets, &c.

3. Place the patient gently on the face, with one arm under the forehead, so that any fluids may flow from the throat and mouth; and, without loss of time,—

I.—*To Excite Respiration,—*

4. Turn the patient on his side, and

(i.) Apply snuff or other irritant to the nostrils.

(ii.) Dash cold water on the face previously rubbed briskly until it is warm.

If there be no success, again lose no time; but,—

II.—*To Imitate Respiration,—*

5. Replace the patient on his face; (when the tongue then will fall forward, and leave the entrance into the windpipe free;) then,—

6. Turn the body gently but completely, *on the side and a little beyond* (when inspiration will occur,) and then on the face, making gentle pressure along the back, (when expiration will take place,) alternately; these measures must be repeated deliberately, efficiently, and perseveringly, fifteen times in the minute, *only*; meanwhile,—

III.—*To induce Circulation and Warmth,—*

continuing these measures,—

7. Rub the limbs *upwards*, with firm pressure and with energy, using handkerchiefs, &c., for towels.

8. Replace the patient's wet clothing by such other covering as can be instantly procured, each bystander supplying a coat, waistcoat, &c.

These rules are founded on physiology; and, whilst they comprise all that can be immediately done for the patient, exclude all apparatus, galvanism, the warm bath, &c., as useless, not to say injurious, especially the last of these; and all loss of time in removal, &c; as fatal.—*London Lancet.*

On the evils of Consanguinity in Marriage. By Dr. RILLIET of Geneva.

A letter from the author, containing a statement of his researches in reference to the influence exercised by consanguinity upon the offspring of marriage, was read before the Académie de Médecine at their sitting of the 13th of May. The substance of the letter was: that at Geneva a considerable number of marriages take place between relatives; that attention has during many years been attracted to the unhappy consequences resulting from this circumstance, and affecting the health and even the lives of the children. These consequences are, first, absence of conception; 2nd, retardation of conception; 3rd, imperfect conception, (miscarriage;) 4th, imperfect offspring, (monstrosities;) 5th, offspring more specially liable to diseases of the nervous system, and in order of frequency: epilepsy, imbecility or idiocy, deaf-mutism, paralysis, various cerebral diseases; 6th, a lymphatic offspring predisposed to the diseases which spring from the scrofulo-tuberculous diathesis; 7th, an offspring which dies at an early age, in a larger proportion than children born under other circumstances; 8th, an offspring which, if it passes the first period of infancy, is less capable than others of resisting disease and death. To these rules there are exceptions, due either to the state of health of the progenitors, or to the dynamic conditions in which the parents happen to be at the time of connexion. Thus: 1, it is seldom that all the children escape the evil influence; 2, in the same family some are affected, others are spared; 3, those who are affected are almost never similarly circumstanced in the same family—that is to say, one is epileptic, while another is a deaf mute, &c.—*Dublin Med. Press from Gazette Medicale.*

On the Effect of Belladonna in Arresting the Secretion of Milk. By R. H. GOOLDEN, M. D.

As nothing is read with greater interest by practical men than your reports of clinical facts, I hope I may claim a corner in your journal, at as early a date as convenient, to relate the following cases, illustrative of the effect of belladonna in arresting immediately the secretion of milk.

E. J——, aged twenty-eight, was admitted into Anne's Ward, St. Thomas's Hospital, with severe rheumatic fever. She had been ill four days, with a child at the breast four months old. At the time of her admission she had swelling and acute pain in both wrists, right elbow, both knees, and left ankle. The knee-joints were distended with synovia, and erythematous patches were on the skin of the knees, ankles, and wrists. She was bathed in perspiration, and the secretion of milk was abundant. According to the regulation of the hospital, the child was removed; indeed, from her helpless condition, it was necessary, considering the difficulty of attending to an infant in a ward with other patients. Soon after her admission she took eight grains of calomel and a grain and a half of opium, followed by a senna draught; and one scruple of nitrate of potassa, ten grains of bicarbonate of potassa, and half a drachm of spirit of nitric ether, in peppermint water, every four hours. The joints were covered with cotton wool.

On the following day, at two o'clock, I found she had been freely purged; the joints were in nearly the same state. She had had no sleep. The breasts had become tumid, hard, painful, knotty, and extremely tender. The superficial veins were distended. Some milk had been drawn, but the process was attended with great pain, and we could not listen to the heart's sounds on account of the tenderness.

A milk abscess, in complication with rheumatic fever, was of all things to be avoided, and unless the secretion could be at once arrested, it appeared inevitable. In this state I recollected that I had somewhere met with an observation (but I cannot remember whether it was in an English or foreign journal) that atrophine applied externally to the breasts would dry up the milk; and thinking it reasonable, I caused the areola of the breasts to be smeared with extract of belladonna, in the same way that it is used to dilate the pupil of the eye. I likewise ordered the addition of half-drachm doses of colchicum wine, knowing that whenever milch cows eat the meadow saffron in the pasture, they immediately become dry; and though I have not much faith in colchicum as a remedy in rheumatic fever uncomplicated with gout, there could be no objection to its use, and it has the sanction of much higher authority than my own.

On my third visit the following day, the first inquiry was about the breasts. They were all right. But was it the colchicum or belladonna that had relieved them? The extract was used before I left the ward; before the mixture was given, the secretion of milk had been arrested and the breasts had become soft. The rest of the case has no further special interest. I will only state that there was no heart affection, and that the fever, though very severe while it lasted, was of short duration, and the patient left the hospital quite well in fourteen days.

The second case that occurred to me was uncomplicated with any disease, and such as would usually fall under the care of the accoucheur rather than the physician:—

A lady, the wife of a clergyman, was travelling with her husband, and in order to accompany him, had weaned her baby, (then seven months old.) Happening to be at Oxford at the commemoration festival, he came to me in great trouble, telling me that his wife had done a foolish thing in weaning the child, and that they were now arrested in their progress in consequence of the state of her breasts. They were tumid, very tender, painful, and hard, with large superficial veins, and the milk had been drawn with difficulty several times, with temporary relief. I recommended the application of the extract of belladonna to the areolæ, desiring them to send for a medical practitioner if the inconvenience did not immediately subside, or unless she felt quite well. A few days brought me a letter, giving a very satisfactory account, and thanking me for what she was pleased to call my wonderful prescription. Within two hours she was perfectly relieved, the milk absorbed, and (what is very important) there was no fever or other inconvenience attending the sudden suppression of the milk; and instead of taking the opening medicine I had prescribed for her, she continued her journey the next morning.

I have not been able to discover that the fact that belladonna is

available for the purpose of arresting the milk secretions is at all generally known—certainly it was not to several accoucheurs in large practice of whom I have inquired. The fact is important, if true, for then milk abscesses will become a matter of past history, and probably many diseases of the breast may be rendered less complicated by its use.

The two cases I have detailed are not sufficient to prove that it will always be either successful or safe, but they render it highly probable that it is so. My assertion may have a temporary interest, and soon be forgotten, and the opportunities of observing milk abscesses, and their early progress, do not occur with such frequency to a hospital physician, even in private practice, as that I may hope to bring together a sufficient number of facts to lay them before you. The fact has already been noticed, and if you will invite others who have more opportunities of special observation to try the experiment, and give you *short extracts* of cases bearing on the subject, with the names of observers, I am sure you will confer a favor on the profession.—*London Lancet*.

Homœopathy.

Men are familiar with the advertisements of disguised medicines which continually appear, and sink into neglect, again to be brought forward under a new name, and perhaps slightly altered in appearance. It is now difficult to find a person who credits what is urged in favor of any one of these nostrums; and they are used, for the most part, when a cheap article is made choice of, such directions as go with them being cheaper than the advice of a physician.

They manage such things differently in Germany. Besides the usual extravagant pretensions, each one is there especially anxious to show that his own nostrum is in the height of fashion, and is used by the nobility everywhere. They endeavor to get the recommendation of some prince or baron, and sometimes with success. The same course was followed by those charlatans who got up the system of medical practice called homœopathy; and when it was imported into this country, that course being continued here, some of the American "aristocracy" were a little, a very little, imposed on thereby. But if any wish to inquire, they can find that homœopathy has for sixty years been scouted by nearly all persons of the higher ranks in Europe; and that the only evidence that it ever was fashionable, is derived from some homœopathic source. The kind of evidence that a lawyer delights in, is that which is wrung from the witnesses of his opponent; and we in this paper rely chiefly on the writings of the homœopathists themselves. Their periodicals are all in reality quack advertisements, under the disguise of scientific publications, as any well-informed person may ascertain by examination. Though no more credible than such advertisements in other cases, especially when they speak of their success, yet that which they contain adverse to their own pretensions is not unworthy of observation. They continually complain that they are subjected to the laws against quackery, which exist in most European countries, and are enforced more or less strictly in different places.

From the *British Journal of Homœopathy*, published at London, Oct.,

1853, p. 665, it appears that no homœopathist ever practised in the large republican city of Frankfort before 1848; that one who began business there, about that time, was soon expelled from the city by the magistrates, solely on account of his method of practice; that he then took up his residence in a neighboring village, within the bounds of Hesse Cassel, and visited his patients in the city as before; and that for these visits he was fined and compelled to leave the neighborhood. From the silence of subsequent publications it would seem that no homœopathist has been permitted to practise there since.

In the same quarterly, for July, 1853, p. 485, it is said that homœopathy was never practised in Rome until Dr. Wahle removed there in 1843, after his residence at Leipsic had been made disagreeable by "persecution;" and that for a long time he met with great difficulties in getting permission to begin business, though latterly his practice was large. It appears from the article that the time when those difficulties were removed, and his practice became large, was at the setting up of the last Roman Republic in 1848. At the restoration of the Pontifical government, his practice seems again to have been interfered with, for it is mentioned that he had leisure to travel into the north and Germany.

In the *North American Homœopathic Journal*, published in New York, May, 1852, p. 127, speaking of late political events, it is said that in France, Italy and Germany, revolutions and still more reactions, have hindered the progress of homœopathy; and that tyrants, at the instigation of their court physicians, will not foster it. From this it should perhaps be inferred, that after the revolutionary movements of 1848 were subdued, the homœopathists were again subjected to the degradation from which the outbreak had relieved them, and that the laws against them were executed more rigorously than before, because some had neglected homœopathy to meddle with state affairs. In the *Quarterly Homœopathic Journal*, published at Boston, July, 1850, p. 300, it is said that Dr. Wurmb, one of the first homœopathists in Vienna, was a captain in the students' legion, during the revolution, and took part in one of the battles; and that the reaction has nearly destroyed him.

The *Philadelphia Journal of Homœopathy*, for Feb., 1854, p. 688, says that homœopathic practice, after a comparatively free course in Bavaria for twenty years, was interdicted in 1842 in all the public institutions of the kingdom; and that in 1848, the year of the revolution, the prohibition was removed, but was again imposed soon afterwards.

The *British Journal of Homœopathy*, for April, 1855, p. 328, says that homœopathy was prohibited in Austria in 1819; and though that prohibition was said to have been removed a long time afterwards, their journals also say other things inconsistent with that assertion. The *Quarterly Homœopathic Journal*, for July, 1850, p. 305, speaking of Austrian tyranny since the revolution, says, "the rescript of the government, permitting the homœopathic practice to all physicians, has never been allowed to be published." That rescript would seem to have been the product of the revolution, and to have lost its force when the emperor recovered his power; and, it is said, on the same page, that some homœopathists, "in order not to be discovered, pretend to prescribe allopathic medicines which their patients never take, using homœopathic

medicines all the time according to the actual directions." Instead of appearing openly, as men of fashion like to appear, they are obliged to hide their doings from the police. Still, it has repeatedly been written that homœopathy is more flourishing in Austria than on any other part of the European continent; and since they are reduced to such expedients, how must they manage in places where they are yet more restricted? As astrologers and other fortune tellers have managed to lurk in the shade when their arts are prohibited, so homœopathy has by some means prolonged a degraded existence without being able to convince the magistrates that its pretensions are true. Were they true, it must have been eagerly received by all, more than fifty years ago. It is now practised by only a few hundred in all Europe, while of regular physicians there are hundreds of thousands.

The *North American Homœopathic Journal* for May, 1853, p. 271, contains an extract from the *Zeitschrift für Homœopatische Klinik*, published in Germany. It gives an account of the state of homœopathy in 1852. It recognizes its low state in Prussia, and other northern parts of Germany; also in France, Sweden, Denmark and Russia, but says it flourishes in Austria, Bavaria and other southern countries of Germany. It says that in England it flourishes more than in Germany itself; that the number of practitioners is there large, and that, "above all, America seems to have given it the warmest embrace." The *British Journal of Homœopathy* for July, 1853, p. 480, speaking of a register of the names of their practitioners in Europe and America, says "the latter, especially, is a most formidable list." We have here important data. The United States, it would seem, contain more homœopathic practitioners than Europe, England more than Germany, and the south of Germany more than the north.

The *North American Journal of Homœopathy* for Nov., 1852, p. 493, contains a list of their practitioners in New York and the principal Atlantic cities. The editors show an anxiety to make the number appear as large as possible. The number given for New York city is 62; for the rest of the State, 242; Philadelphia, 53; Boston, 20; Providence, 9; Baltimore, 10; Washington, 2. Total, 398. This is the list so much admired by the Europeans; and as in the scattered population of the south, and of the country towns of the west and the east, they fail in finding adherents sufficient for support, there is no reason to suppose that one hundred others could have been reckoned in this country.

The *Quarterly Homœopathic Journal* for April, 1850, p. 285, quoting the *British Journal of Homœopathy* for the January preceding, gives a list of the British practitioners. The number is 48 in London, 51 in the rest of England, 10 in Scotland, and 7 in Ireland. Total, 116. Though the number in Germany they thus show to be insignificant, no credible evidence is found that it ever was larger. The Central Homœopathic Union of Germany, which collects to its meetings as many as possible of the practitioners of that country, held its annual meeting in 1854 at Weimar, in a densely populous region traversed by railroads. The number present was published, for it seems to have been unusually large. It was twenty-seven, of whom two were styled apothecaries, and

one a veterinary surgeon. The year previous, the meeting was appointed at Hesse Cassel; but the homœopathists were prohibited by the sovereign of that country from assembling within his dominions.) See *Brit. Jour. Hom.*, Oct., 1853, p. 668; and Oct., 1854, p. 683.

As men of eminence have in some instances resorted to homœopathy, in times of doubt and terror, so they have resorted to dealers in false pretensions of other kinds. The last-named journal for Oct., 1853, p. 670, publishes a certificate alleged to have been signed by the Marshal de St. Arnaud, declaring that he had just been completely cured, by homœopathy, of a disease that for fifteen years had seriously troubled him. The disease referred to must have been the one which soon afterwards proved fatal to him, at the close of the battle of Alma. The same page contains his answer to a request that he would use his influence to relieve the homœopathists from the legal disgrace that burdens them in France; but though he was then Minister of War to Louis Napoleon, he with some ambiguous, courtly expressions of regard, declined to take the first step in the matter. Though he consulted them as he might have consulted fortune tellers, he seems to have held them in no higher estimation.

The degree of success, which has attended them in this country, has resulted chiefly from their groundless assertions that they are everywhere patronized by the most fashionable, the most intelligent, and the most learned; and in fact some of the American literati appear to have been rather easily captivated by their elegantly printed publications, and even half convinced by their external appearance alone. There is a sort of natural philosophy, admired by many, called transcendentalism, whether correct or otherwise it is not to the purpose here to inquire; but the chief of the works on homœopathy are written in the style of transcendentalism, and contain many of its peculiar expressions and modes of thought. Yet it is not transcendentalism, but an imitation of it merely; so, of course, the German metaphysicians consider it, or they would not have despised it, as they all do, except some few of very peculiar acuteness of penetration. Had they received it, the practice of it would never have been forbidden. It is not only counterfeit science, and counterfeit fashion, but counterfeit transcendentalism. The American votaries of this philosophy have, some of them, been prone to take the counterfeit for genuine; and it is not rationally to be expected that such as have resided not many weeks in the neighborhood of German libraries, and are but moderately well versed in the things of Germany, can have developed those powers of detecting German counterfeits, which the natives of the land themselves possess.—*Boston Medical and Surgical Journal*.

Wounds of the Abdomen. BY DR. ROBARDS.

I was called, during the coldest weather in December, in consultation with Dr. Peyton, of this place, to visit two men that had been severely wounded in a fight that took place on a Ferry Boat, while crossing Loosa Hatchie, about four miles above the city. On our arrival, we found both

the patients in a small log house, occupied by the ferryman. One, Mr. B. was lying on the bed, bleeding profusely from various wounds in different parts of the body; the most serious of which, however, was one just above the elbow joint in front, which severed the whole of the integuments down to the bone. The other patient, Mr. S. was lying on the floor before a large fire, apparently in a dying condition. He also had received numerous wounds, inflicted with the same knife, which, though not seen by us, was supposed to be an ordinary large dirk knife, with a blade $\frac{3}{4}$ of an inch wide, and six or eight long. The wound in this case that was of the most importance, and seemed inevitably fatal, was in the abdomen—on the left side, a little below the umbilicus. A large amount of intestine and omentum was protruding, and resting upon the floor. It must have been inflicted at least an hour and a half before we saw it, and on inquiry we found that after receiving the wound he jumped into the river, and either swam or waded ashore, with this amount of intestine and omentum hanging out (it could not have been less than a hat-full,) and the thermometer standing considerably below freezing point. On reaching the shore, he laid some time on a log, and was then taken into the house by some friends. Being in a state of collapse when I examined him, pulse feeble, the extremities cold and clammy, the features contorted and palid; he was let alone to die, and our attention directed to Mr. B., whose wound occupied $\frac{3}{4}$ of an hour in dressing. In the meantime, however, S. frequently called for aid, begging us to give him morphine and let him die easy, that he knew he was obliged to die, &c. He was given large doses, but no brandy—he had taken quite enough of that before the fight. Finally after becoming a little excited and relieved by the morphine, he began to beg to be dressed, and said no one knew but that he might get well yet. I asked him many questions while still engaged in dressing the wounds of the other patient, among them was, what kind of a pistol he shot his adversary with. He replied, “a d—d good one.” (I mention this fact to show what powerful influence the mind and nerve can exercise in a case of severe wounds of any kind.) In my opinion, nine-tenths of the men in the world would have died of similar wounds and under similar circumstances, in three hours.

Finally, my attention was directed to the dressing of his wounds, more for decency's sake than with any hope of saving him. The intestines, though wounded in various places, had only one cut of any considerable size; the others were merely punctured, and completely obstructed by the protrusion of the villous coat. The distention of flatus was enormous; strangulation being perfect. The bowel and omentum were carefully placed in a basin of warm water, cleansed and softened, (they had become completely glazed) the large wound was closed with the Glover's Suture, the small ones let alone. I then introduced a grooved director, and enlarged the wound in the abdominal walls about one-half; by gentle manipulation, returned the bowel the reverse of the manner of its escape, and finally the large quantity of omentum. The wound was brought together and confined, by means of two or three interrupted sutures, a compress and bandage being applied. I proceeded

to examine various other incised and punctured injuries in different parts of the body. One was immediately over the liver, and penetrated the peritoneal coat; but as there was little or no hemorrhage, I presume it did not enter that organ. Finally the patients were given an additional opiate and allowed to rest. The next morning I called to see those patients, and to my surprise, I found both without a bad symptom. Reaction had come on, but not too much. The bowels were kept confined in S.'s case; antiphlogistic remedies recommended, and I saw no more of them, Dr. Peyton taking charge of the cases. Two or three weeks afterwards I learned that S. was at the County seat, Raleigh, ready for another bout with any man that crosses his path.

This case is reported to show the extraordinary recuperative powers of nature when backed by indomitable resolution and a will of iron; and furthermore, that we should never desert a case in Surgery, as long as there is the evidence of remaining vitality, and there is no vital part mortally wounded.—*Memphis Medical Recorder*.

PARIS, June 30, 1856.

M. Baudens, Surgeon-in-chief of the French army in the East, has written a letter to the President of the Academie des Sciences, on the Typhus of the Crimea. He examines the differences between this typhus and the typhoid fever as it exists in France, and he concludes that these two diseases are essentially different one from the other; contagion, doubtful for typhoid fever, cannot be doubted for typhus. At the *Ambulance* of the first division of the army, almost all the Physicians and nurses, and nearly all the soldiers, admitted for other diseases, had typhus; fifteen Physicians out of sixteen had it. Between the Crimea and Constantinople, thirty-seven physicians, twenty sisters of charity, eight clergymen, many hundred nurses, all in full health, died from having been exposed to contagion. The incubation of the disease lasted about six days. The Secretary of Dr. Baudens was attacked seven days after having visited an Hospital. The causes of the typhus in the Crimea acquired a great power after the 1st of January, 1856, because then, the soldiers, on account of the cold, had shut themselves up in their tents, the ground of which was damp, and filled with impure matters.

A fact observed in France, upon soldiers coming from the Crimea, shows that the incubation of typhus may be much longer than the average fixed by Dr. Baudens. A regiment left Balaklava on the 29th of April, and came direct to Marseilles, where it landed on the 10th of May. From Marseilles it went up to Calais without any case of typhus; but, from thence, and after May 24, the regiment, travelling on foot, had to leave patients in all the places it passed through. At Neufchateau, ten soldiers attacked with typhus were left apparently very dangerously ill. Nevertheless, they all recovered, very likely because they were not exposed to the causes of the typhus, and were taken care of in every respect much better than the patients treated in the Crimea or at Constantinople. It results from this fact that the incubation of typhus may be longer than twenty-five days (from the 29th of April to the 24th of

May.) The details on this subject are related in a letter of M. Garcin, read at the Academie des Sciences on the 16th of June.

A very remarkable paper, on the power of adaptation of the eye to different distances, has been presented to the Academy, by M. Charles Rouget. After having given a new description of a part of the ciliary muscle, recently discovered by a pupil of Professor Donders, M. Rouget shows that the muscular fibres of the iris are a direct continuation of the fibres of this part of the ciliary muscle. He states also that the part already known of the ciliary muscle (*i. e.*, Bruecke's muscle) is in continuity on one side with the choroid, and, on the other side, with the membrane of Desceinet. These anatomical facts being well understood, it is easy to admit that a combined contraction of the ciliary muscle, and of the iris, will increase the antero-posterior diameter of the dioptrical apparatus of the eye. M. Rouget explains all the details of these phenomena, and also of many others, which, according to his anatomical researches, take place at the same time, and among these, particularly, a kind of erection of the iris, produced by the compression of its veins. In the last part of his paper, M. Rouget shows how the results of his researches agree with the important experiments of Krasmer, Helmholtz, and Donders.

An interesting discussion has taken place at the Academie de Medecine on the relations between the horse-disease called *grease* and small-pox. It is well known that Jenner thought that grease was the origin of cow-pox; although, in the only experiment he tried on this subject, he did not see cow-pox produced in a cow after the inoculation from the matter of grease. A case reported to the Academy by M. Bousquet seems to be favorable to the opinion of Jenner. A man having shod a horse attacked with grease, had, twenty-four days after, a vaccine eruption over his hands. The liquid of the pustules, inoculated to some children, produced true vaccine pustules. Another fact, almost of the same kind, was observed in Paris in 1805, on a driver. M. Leblanc, a learned veterinary surgeon, objects to the views of Jenner, and to the conclusions which M. Bousquet drew from the preceding facts. He relates many experiments, made by himself or by M. Bousquet, in which the liquid of grease, having been inoculated either in cows or in men, did not produce anything like vaccine. It seems that M. Leblanc, M. Bousquet, and some other Academicians, did not know fully the state of science on this subject. In an article published in the *Gazette Medicale de Paris*, (No. 24, 14th of June,) M. Chlozan shows that many experiments made by Lay, a physician of Pickering (Yorkshire,) and by Sacco, prove that in some circumstances the matter of grease may produce true cow-pox. These experimenters have also seen a few cases of production of vaccine in men who had been touching horses attacked with grease. Jenner relates two cases of this kind; and he says that the cases in which inoculation of grease to cows had produced no specific pustule, prove nothing, as it is well known that the inoculation of true cow-pox on a cow does not produce cow-pox.—*Letter to Med. Times and Gazette*, July, 1856.

Abstract of Meteorological Observations for July, 1856, made at Philadelphia, Pa. Latitude 39° 57' 28" N., Longitude 75° 10' 40" W. from Greenwich. By PROF. JAMES A. KIRKPATRICK.

1856. July.	BAROMETER.		THERMOMETER.		SOLAR RADIATION.		REL. HUMID. 2 P.M.	FORCE OF VAPOR 2 P.M.	DEW POINT 2 P.M.	RAIN Inches.	PREVAILING WINDS.	REMARKS.
	Daily Mean	Mean Range.	Daily Mean	Mean Range	Ex- treme Range	Deg. 2 P.M.						
1	29.938	.339	78.0	12.5	13.0	50.0	28	.327	47.3		N.W.	Clear.
2	29.956	.059	75.8	2.5	10.0	14.0	44	.450	56.0		(Var.)	Cloudy. <i>Barometer highest 29.998.</i>
3	29.737	.219	80.0	4.2	13.5	25.5	50	.608	64.6		SW.	Cloudy.
4	29.608	.128	78.2	2.8	10.0	26.0	50	.582	63.3		(Var.)	Morning and aft. cloudy; 12 M. until 2 P.M. a few drops of rain; evening clear.
5	29.707	.099	77.0	2.5	18.0	44.0	32	.354	49.5		E.	Clear.
6	29.667	.041	79.0	2.7	12.0	8.0	38	.478	57.8	0.054	SW.	Morning clear; aft. cloudy; 5 1/4 P.M. thunder, 5 1/2 rain until 7 1/2.
7	29.758	.022	77.5	2.8	14.0	28.0	66	.635	65.8	0.036	NE.	Cloudy; 5, P.M. until 5 1/2, rain.
8	29.814	.056	76.7	3.2	8.0	3.0	59	.623	65.3	0.182	SE.	Cloudy; morning and afternoon drizzling rain.
9	29.841	.027	73.2	2.8	9.0	16.0	69	.638	66.0		(Var.)	Morning and aft. cloudy; evening clear.
10	29.881	.045	73.7	3.2	13.0	47.0	58	.573	62.9		NE.	Morning and afternoon cloudy; evening clear.
11	29.728	.153	73.7	2.7	17.0	20.0	59	.640	66.4	0.709	E.	Cloudy; afternoon and night, rain.
12	29.588	.139	77.7	3.0	15.0	10.0	63	.717	69.3		(Var.)	Morning and afternoon cloudy; evening clear.
13	29.699	.111	81.3	3.7	8.0	5.0	75	.908	76.4	0.073	SW.	Cloudy; 11 1/2, A.M. rain, with thunder for an hour.
14	29.777	.078	83.3	4.0	15.0	5.0	48	.697	68.5		SW.	Morning and afternoon cloudy; evening clear.
15	29.625	.152	84.8	2.5	12.0	—	53	.750	70.7		S.	Cloudy; 11 1/2, A.M. rain, with thunder for an hour.
16	29.715	.089	81.0	5.2	21.0	17.0	34	.451	56.1		(Var.)	Morning cloudy; afternoon and evening clear.
17	29.648	.067	85.3	4.3	30.0	42.0	36	.595	63.9		SW.	Morning cloudy; afternoon and evening clear.
18	29.512	.135	89.0	3.7	24.0	31.0	31	.655	61.9		SW.	Clear. <i>Thermometer highest 100°.</i>
19	29.563	.081	75.7	13.3	6.0	20.0	39	.374	51.0		W.	Morning and evening clear; afternoon cloudy.
20	29.670	.107	73.3	3.7	19.0	19.0	31	.314	46.3		N.W.	Morning and evening clear; afternoon cloudy.
21	29.739	.070	75.7	2.3	26.0	42.0	35	.441	55.5		N.W.	Clear. <i>Thermometer lowest 60°.</i>
22	29.800	.063	77.3	1.7	27.0	42.0	31	.414	53.8		N.	M. and ev. clear; aft. cloudy; 6 P.M. a few drops of rain.
23	29.805	.005	79.7	2.3	31.0	46.0	26	.374	51.0		(Var.)	M. and ev. clear; aft. cloudy. <i>Therm. lowest 60°.</i>
24	29.792	.013	83.7	4.0	30.0	50.0	30	.486	58.2		SW.	Clear.
25	29.722	.070	83.8	1.8	28.5	32.5	36	.545	61.5		WSW.	Clear.
26	29.703	.042	86.0	2.8	33.0	40.0	37	.624	65.3		WSW.	Clear.
27	29.630	.073	86.7	1.3	33.0	29.0	32	.568	62.6		NW.	Morning and afternoon cloudy; evening clear.
28	29.650	.021	86.0	2.7	26.0	27.0	31	.512	59.7		NW.	Morning and evening clear; aft. cloudy; 6 P.M. high wind.
29	29.685	.063	80.7	5.3	18.0	12.0	57	.689	68.2	0.073	(Var.)	Cloudy; 8 1/2 to 11, A.M., rain at intervals.
30	29.462	.223	84.8	6.5	21.0	40.0	51	.767	71.3		NW.	Cloudy. <i>Bar. lowest 29.447.</i>
31	29.562	.100	81.0	3.8	14.0	39.0	32	.437	55.2		NE.	Morning and afternoon cloudy; evening clear.
Means for July, 6 yrs.											S. 85° 22' W. 25-100.	
											S. 77° 30' W. 12-100.	

The Monthly Range of the Mercury in the Barometer was 0.551 of an inch, and in the Thermometer 40°.